SOME PROBLEMS OF MEASUREMENT AND INTERPRETATION IN THE STUDY OF DARK ADAPTATION

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I. INTRODUCTORY

In a previous paper(28) the present writer described an investigation of the possibility of raising the normal level of sensitivity of the dark adapted eye by means of preliminary stimulation with coloured light. An alternative method of increasing sensitivity which had been suggested was the addition of vitamin supplements to an already satisfactory diet. A short series of experiments on these lines will be discussed, but the present paper is mainly concerned with a number of subsidiary problems, mostly dealing with matters of interpretation which have arisen during the course of a series of studies of dark adaptation (hereinafter D.A.). Included is a brief consideration of certain points, arising from the published reports of other investigators, which the writers themselves have not taken up.

II. PROBLEMS OF MEASUREMENT

(1) *The concept of normality of* D.A. Any consideration of normality in respect of D.A. is inevitably bound up with the interpretation put upon the term D.A. itself. The term is somewhat loosely used to designate both the process whereby the sensitivity to light stimulation of an observer working under conditions of darkness increases, and also the end-state of this process; thus D.A. may be said to be 'complete', or the observer fully dark adapted, when maximum sensitivity has been attained. This is now generally believed to coincide with the complete regeneration of visual purple previously broken down under the influence of light stimulation. The possibility of this effect being masked by psychological causes has been discussed by many writers, and it would appear that 'ability to see in the dark', as measured by the determination of visual thresholds or otherwise, cannot be taken as a direct or infallible indication of the stage reached by physiological changes in the retina, although in a large proportion of cases, especially those of extreme deficiency, the relationship does seem to hold good. Even from a more purely psychological standpoint, however, one's concept of normality depends to a large extent on the purpose to which results of adaptometer tests are to be put. Such results may be affected by any or all of a large number of factors, some of which (e.g. eye colour, age, etc.), if operative, are obviously unalterable, while the effects of others, again if operative, may possibly be overcome (e.g. nutritional condition). Briefly, then, one's standards of normality alter according to whether one is measuring

an individual's sensitivity in relation to his own maximum, or in relation to the average sensitivity of the population at large.

(2) *Variations in testing technique.* A still more fundamental point to be considered is the influence of testing technique. Groups of researches based on the same instruments and methods exist, but on the whole there are astonishingly wide divergences as between groups, not to mention individual workers. Much depends on the instrument, and while certain adaptometers have been devised to meet special requirements, some of these have been later used for purposes to which they are not necessarily adapted. A survey of some of the more modern instruments is given in a Crookes' Laboratories publication(2), but in view of rapid developments this is naturally incomplete. The following summary of variations in methods of testing D.A. (for practical as distinct from research purposes) includes those dependent both on the instrument and on the interests or preferences of individual investigators:

(*a*) The time allowed for actual D.A. varies between 150 sec.[1] (Ditchburn(9)) and 24 hr. (Achmatov(1)). For clinical and diagnostic purposes the time is usually between 10 min. and 1 hr.; the longer periods have been used mostly for research purposes.

(*b*) In present-day practice a period of preliminary light adaptation, sometimes termed 'pre-adaptation' or 'bleaching', is almost universal. The duration of such adaptation, and the intensity of the light used, vary considerably, and in some cases the pre-adaptation period is itself preceded by a preliminary D.A. period. Opinions differ as to the effects of duration and intensity of the pre-adaptation light, but the object is of course to standardize the condition of the eye before D.A. proper begins; ideally, full light adaptation will have been attained.

(*c*) The test light may be viewed with one eye or with both. The relation between monocular and binocular thresholds must therefore be determined. Considerable complications may arise through even the most ordinary visual aberrations; it seems probable that insufficient attention has been paid to the psychological aspects of such abnormalities. Thus, for example, a marked amblyopia, though insufficient to destroy vision in the affected eye, may habitually inhibit its use, so that one cannot be quite certain to what extent binocular vision actually does occur in such a case.

(*d*) The judgement required of the observer may depend on pure perception of a liminal stimulus, or on discrimination of form. Thus, different methods ((11,29)) require the observer to state the direction in which the 'arms' of a capital E are pointing, or where in the circumference of a circle a break is located.

(*e*) Variations may also occur in the size of the test field, as measured by the retinal angle it subtends, and in the position of its image in the retinal field; both points are fully treated by Hecht, Haig & Wald(15). Control in these respects is of particular importance in view of established differences between foveal and peripheral adaptation. The former reaches its limit more quickly than the latter, and this limit represents a much smaller increase in sensitivity than that shown by full peripheral adaptation.

(*f*) The observer may be required either to make judgements at stated intervals, or to remain continuously on the alert, reporting when a given stimulus becomes perceptible. A compromise is seen in methods, such as that of Wald & Clark(33), in which the test

[1] A still shorter time is used by Pett(26), who measures the time taken 'to see a dim light of constant intensity'; the range found is 3 to 50 sec.

light is flashed on and off continuously, with adjustments in intensity as successive stages of sensitivity are reached and passed.

Other points of technique (e.g. control of brightness and colour of both test and pre-adaptation lights, use of an artificial pupil, etc.) also arise, but these are more specifically relevant to theoretical and research problems, and fall outside the scope of this paper. A survey of some of these is given by McDonald(23), who stresses their importance in connexion with aspects of the D.A. process other than its final threshold value.

III. PROBLEMS OF INTERPRETATION

(1) *Treatment of data.* In addition to the above variations in experimental technique, many differences have occurred in what may conveniently be termed *interpretation*, using the word in its widest sense.

(*a*) The most obvious of these differences is in method of plotting adaptometer readings to produce an adaptation curve. This is more important than might appear at first sight, since on the choice of method of plotting depends one's idea of the course of D.A. (i.e. at which stages it is slow and at which rapid), and also of what constitutes a significant or 'sizeable' difference between individual curves.

It was early recognized that direct plotting of successive threshold intensities against time not only produced unmanageable results but was also misleading, since it obscured significant changes in threshold, which differed enormously at different stages of the D.A. process. The most obvious alternative was to plot reciprocals of threshold intensities, and this led to the view that D.A. proceeded at first slowly, then rapidly, and then slowly again. Experience, however, both of the test situation and of ordinary conditions of darkness, suggests that this description is untrue. Test conditions, at least, show that from the subjective point of view it is more correct to say that D.A. is very rapid in the initial stages, and gradually becomes slower. Curves in agreement with this observation were first obtained by Best(3), who plotted in powers of 2, and this is what in effect is done when, as in the Rowett method, successive apertures of an adaptometer are each half the area of the preceding one. It is now more usual to plot logarithms of threshold intensities (or of their reciprocals), which gives the same results; the direction of the curves—*down* for log thresholds, or *up* for reciprocals—is purely a matter of taste.

(*b*) Some experimenters have made it their practice to smooth their curves, thereby eliminating or minimizing reversals in threshold value. These are variously regarded as real or accidental, in which latter case they are usually put down to experimental error or errors of judgement. On the other hand, methods of D.A. measurement which make use of automatic recording devices (e.g. Craik & Vernon(8)) have given results which show that a test field of given intensity, visible at one moment, may later temporarily disappear, which would be impossible from the point of view which holds that sensitivity of the dark-adapted eye increases continuously, without reversal, until the process is complete.

(*c*) The possibility of laying down 'standard curves' has been discussed by certain writers, particularly Matthey(24), who has also recognized the existence of different 'types' of D.A. curves. Thus, some subjects seem to adapt relatively more quickly or slowly at different stages of an observation period. Such differences are, of course, obscured if any sort of average curve for a number of individuals is drawn.

(*d*) While a complete curve, particularly if unsmoothed, gives the only true picture of what happens during a D.A. session, it is often desirable or even necessary to be able to represent the results by means of a single number. This leads to the question of the choice of numerical measure. D.A. may be measured in terms of maximum sensitivity reached during a given time, which may or may not coincide with the subject's true maximum, or in terms of time taken to reach a level of sensitivity corresponding to an arbitrarily chosen threshold value. In an earlier paper(28) I used the terms *extent* and *rate* to describe these contrasted measures, though the latter would more properly be applied to a measure which took into account the observer's initial (i.e. light adapted) sensitivity. This latter, however, seems to have comparatively little bearing on practical problems (but see § IV (5)), and it is seldom used, except for purely theoretical purposes.

(2) *Factors affecting adaptometer results.* It is not proposed here to lay down hard and fast recommendations for the unification of methodology in the study of D.A., since variations will continue to be unavoidable or even desirable, following the requirements of a specific investigation. Surveys fall into two main groups according to whether interest attaches to the actual or the ideal, i.e. according to whether an individual's power of D.A. is being assessed with a view to making use of his present capacity for seeing in the dark, or with a view to improving it if deficient. Thus it seems fairly certain that the power to adapt to darkness decreases with age (Ferree & Rand(10)), and the view has even been expressed, e.g. by Trumbull(31), that significant changes take place right from the earliest age at which a child can be tested. Other 'permanent' factors which have been claimed to affect D.A. are mostly specifically ocular in character. It has been shown by Helson & Guilford(18) that eye colour affects only foveal adaptation, and that to a slight degree. Certain hysterical and pathological eye conditions undoubtedly affect adaptometer results (see Scott(35)), but most ordinary visual abnormalities (errors of refraction, etc.) are believed to have little effect. The position with regard to colour blindness is rather obscure. With coloured stimulus lights (see e.g. Best(4)) the colour blind appears to be at a disadvantage, but there seems to be reason to believe that with colourless test fields the colour blind does at least as well as a normal observer.

The principal factor in respect of which alteration may be effected is nutritional condition. A vast literature exists on the subject of the relation between vitamin A intake and D.A., based on experiments of two main types: (*a*) those in which the diet of supposedly subnormal individuals is supplemented by vitamin concentrates, and (*b*) experiments in artificial vitamin A deprivation. Striking results using the latter method have been obtained by Jeghers *et al.*(20, 34) and by Hecht & Mandelbaum(16). In experiments of type (*a*) the results have been less conclusive, and Isaacs, Jung & Ivy(19) have called attention to the fallacies underlying certain methods of diagnosis and interpretation, particularly lack of adequate standards and of sufficient control through the testing and retesting of normals. These writers, and also Palmer(25), have obtained on the whole negative results, while Thomson *et al.*(30) conclude that there is not necessarily a close correlation between D.A. and vitamin A intake in the diet to which the subject is accustomed. On the other hand, an exhaustive study by Jeghers(20) of personal food habits, incidence of 'colds', etc., suggests an opposite conclusion. Thus it would seem that while severe vitamin A deficiency undoubtedly causes impairment of D.A., it cannot be said with certainty that subnormal D.A. is *necessarily* indicative of mild vitamin A deficiency, such as might be associated with an inferior diet. There seems room for con-

siderable further work on the whole question of nutrition in its bearing on D.A.,[1] with special reference to possible variations in individual needs, and on the still more general question of the interrelationships of physiological and psychological conditions, the latter including both personality considerations and sensory discrimination. It should further be noted that, as many writers point out, affected persons are often unaware of their raised visual thresholds; disability in night vision sufficiently serious to be termed 'night blindness' may, at least in some cases, be a totally unrelated phenomenon (see, e.g., Wittkower *et al.*(35)).

Between these two groups of factors affecting D.A., the 'permanent' and those subject to alteration, comes a further complex factor, which may perhaps appear to comprise a number of independent variables. It includes the intelligence of the observer, his 'test sophistication' or responsiveness in the test situation (see Semeonoff(35)), the effects of practice or its lack, and possibly those of changes in the general affective condition of the observer. The D.A. test situation is one of the utmost delicacy, requiring full and careful co-operation on the part of the observer, and it is therefore not surprising if some less-skilled subjects fail to rise from what has been termed their 'cognitive' threshold to the true limits of their sensory discrimination.[2] Similarly, in spite of the purely perceptual character of the situation, and of views expressed to the contrary (e.g. by Thomson *et al.*(29)), the present writer's experience suggests that the practice effect, though erratic, is of considerable importance (see § IV (4)).

IV. SOME EXPERIMENTAL FINDINGS

Although the main purpose of this paper is not to present experimental findings, certain results not published elsewhere are available, and these may be described briefly before attempting to reach any conclusions regarding the possibility of unification of methods in the study of D.A. Except when otherwise stated, the general technique and treatment of results, as well as the identity of the principal observers participating, was the same as in the writer's previous paper(28).

(1) *Effect of frequent presentation.* The view has sometimes been expressed (e.g. by Hecht(12)) that the light stimulation represented by exposure of the test field may, particularly if this is continuous or very frequent, seriously interfere with the adaptation process. A contrary result, on the other hand, has been obtained by Kravkov & Semenovskaja(21), who found a lower degree of sensitivity when readings were not taken until after 60 min. in the dark. To check this point, D.A. sessions were carried out with subjects A and B, taking readings, as far as possible, every minute for the first half-hour, and every 2 min. for the next hour. Fig. 1 shows the results for subject A, along with a curve obtained under 'normal' conditions 3 days previously. It will be seen that there is no appreciable difference between the courses of the two curves, and that two readings at intervals of 8 and 5 min. following the 'frequent' readings, showed little or no change from the previous general level. The results for subject B were substantially the same.

(2) *Fluctuations in* D.A. *curves.* An examination of all curves obtained under different conditions gave the results summarized in Table 1. It will be seen that among 146 D.A.

[1] Thus, Booher(5) states that the composition of diet other than its vitamin A content has been reported to affect the absorption of both vitamin A and provitamin A.

[2] For the same reason, group methods are on the whole unsuited to the measurement of D.A.; they are therefore not discussed in the present paper.

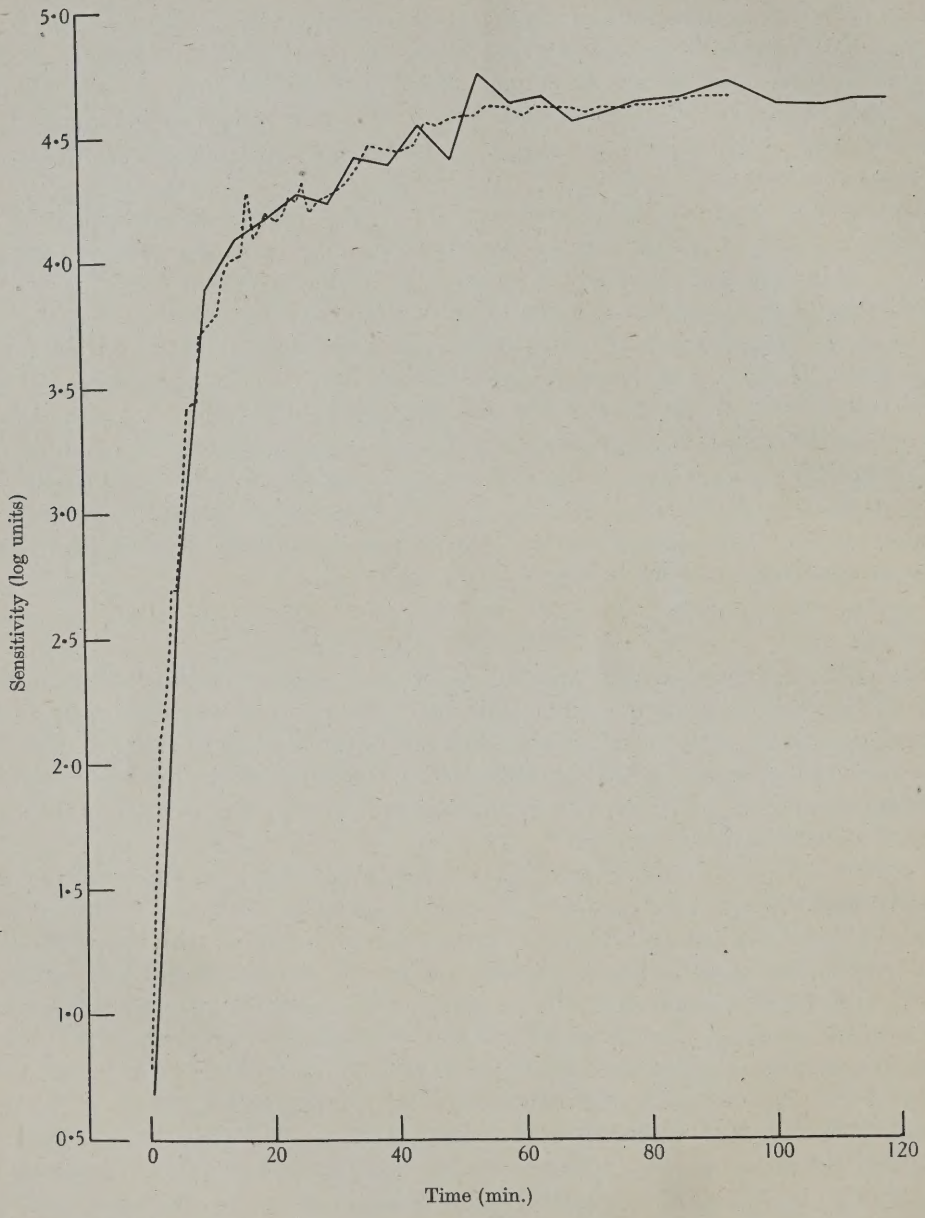


Fig. 1. Effect of frequent presentation.

——— Ordinary conditions ------ Frequent presentation

sessions totalling about 190 hr., curves for only seven (less than 5%) were free from reversals of direction. Whatever their status, therefore, fluctuations in sensitivity certainly do occur, particularly during D.A. sessions of an hour or more.

(3) *Effects of increased vitamin intake.* It was originally intended to extend the experiments here described rather further than was eventually found possible, the intention being to find whether vitamin supplements increased powers of D.A. in individuals who might be considered normal in this respect, and in that of adequacy of ordinary diet. The experiments were carried out during December 1940, following a period of about 6 months during which both subjects had taken part in a very large number of D.A. experiments. For 2 weeks subject B took the recommended daily dose (two capsules) of halibut-liver oil, corresponding to 17,600 i.u. and equivalent to three to four times the essential daily minimum. By way of control, subject A during a similar period took

Table 1. *Fluctuations in curves of dark adaptation*

Subject	Length of session	Total no. of sessions	No. of sessions free of reversals
A	2 hr.	30	—
	1–1½ hr.	6	1
B	2 hr.	23	—
	1–1½ hr.	4	—
	6 hr.	2	—
C	1½ hr.	6	—
35 normal	40 min.	35	3
40 neurotic	40 min.	40	3
	Total	146	7

Table 2. *Effects of vitamin 'treatments'*

Day of treatment	Subject A (vitamin C)		Subject B (vitamin A)	
	Maximum sensitivity	Time (min.) to reach 'level 4'	Maximum sensitivity	Time (min.) to reach 'level 4'
0	4·58	21	4·47	23
1	4·83	19	4·54	22
3	4·77	20	—	—
5	—	—	4·69	22
7	4·64	16	4·78	18
9	—	—	4·76	20
10	5·20	17	—	—
12	5·35	14	4·67	18

daily approximately 150 c.c. of orange juice. The vitamin C content of this may very roughly be reckoned to be equal to about the required daily minimum; it also gives a fair amount of vitamin A, probably about 1000–2000 i.u. D.A. sessions were carried out on the day preceding and at frequent intervals during the 'treatment' period. The results are summarized in Table 2, in which maximum sensitivity and time taken to reach an arbitrary level of sensitivity are both noted. Since this table gives only an imperfect idea of the course of adaptation on the various days of the 'treatment', the significant portions of the smoothed curves are shown in Figs. 2 A, B. In the light of the data given in the table and figures, it will be seen that orange juice gave just as good or better 'results' than did halibut-liver oil, and that very generally speaking, the improvement was progressive over the period studied. Unfortunately war conditions, particularly the impossibility of obtaining oranges, prevented further experiment on the same lines. Accordingly, instead of having the subjects exchange treatments, a further subject C

was given a course of halibut-liver oil, and tested three times before and three times during the treatment period. This subject was himself a psychologist, and fully accustomed to experiments in perception, including D.A., though not to the specific technique

Fig. 2 A. Subject A.

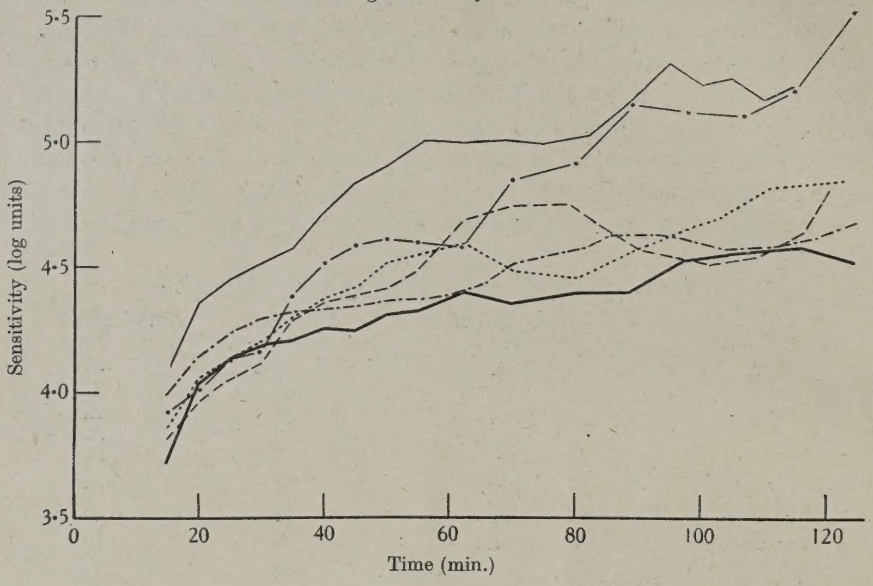
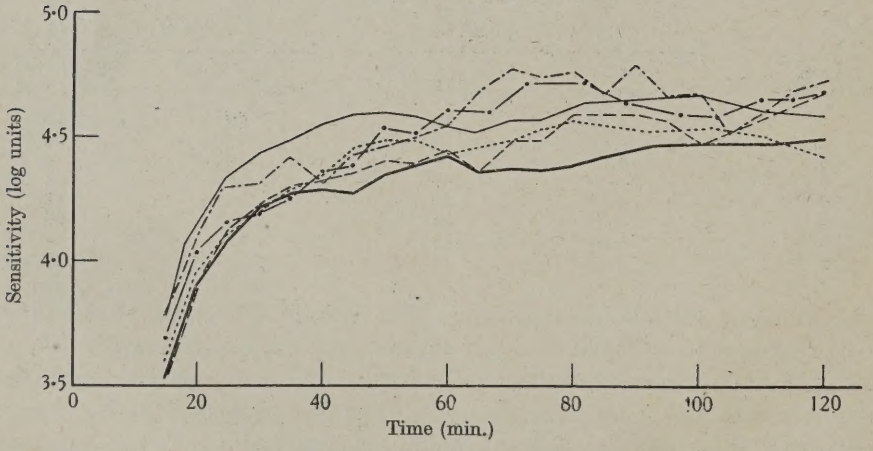


Fig. 2 B. Subject B.



Figs. 2 A, B. Smoothed D.A. curves: before and during treatment with orange juice (subject A) and halibut-liver oil (subject B).

preliminary — 3rd day (A), 5th day (B) — 10th day (A), 9th day (B)
1st day — 7th day — 12th day

employed. His powers of D.A. were reported to be exceptionally low, but judged by the standards suggested elsewhere(35) they would be classed as 'low normal'. His results are summarized in Table 3, and, allowing for lack of practice on the first trial of all, seem on the whole to be negative.

An equally inconclusive result was obtained from subject A when tested before and on the 13th day of a course of 'Multivite' tablets, the dose taken comprising 6000 i.u. daily of vitamin A, and 400 of vitamin C, besides fair amounts of vitamins B_1 and D. Level 4 was reached after about 14 as against 18 min., but maximum sensitivity was 4·70—actually lower than the maximum 4·80 obtained the day before the 'treatment'.

(4) *Effects of practice.* Comment has already been made on the effect of practice on adaptometer results. Since the adequate measurement of D.A. is such a time-consuming process, the task of separating the effect of practice from that of day-to-day variation is doubly difficult, and experience of extended experiments in auditory as well as visual perception has convinced the writer that day-to-day variation is greater than generally recognized (see also Hecht *et al.*(14)). Table 4 shows results of five ostensibly identical sessions, obtained with the same subject (A) at different times. Session 21 seems to exhibit the beneficial effects of practice; on the other hand, session 31, following a fairly long 'idle' period, gives an exceptionally high sensitivity measure. The only conclusion possible is that while practice may improve adaptometer results, its effects are liable to be masked by other factors, whatever these may be.

Table 3. *Effects of vitamin A: subject C*

Day of treatment	Maximum sensitivity	Time (min.) to reach 'level 3'
Preliminary 1	3·50	26
2	3·71	20
3	3·80	20
1	3·72	18
8	3·83	19
17	3·93	17

Table 4. *Variability of results of adaptometer tests*

Serial no. of session as S	Circumstances	Maximum sensitivity	Time (min.) to reach 'level 4'
13	Second session after 2 weeks' break	4·51	18
21	End of month's intensive work	4·72	17
25	Following less intensive period on special problems	4·58	19
31	One month after session 30	4·80	18
33	Two months after session 31	4·65	15

(5) *Possibility of a single measure of* D.A. Although rate of D.A. and the final level of sensitivity reached obviously differ in significance for different problems, it was thought desirable to find how far the various measures obtainable from a single session correlated with one another.

Using the smoothed curves obtained for 40 min. D.A. with the groups of neurotic and normal subjects described elsewhere(35), the following measures were chosen: (*a*) Maximum sensitivity during the 40 min. test period, neglecting any effect caused by a single high final reading. (*b*) Sensitivity at the end of 10 min., the total period used in the work of many American investigators (e.g. Corlette *et al.*(7)). (*c*) The difference between maximum and initial sensitivity. (*d*) Time taken by the smoothed curve to reach 'level 1', i.e. one log unit below the maximum intensity of the adaptometer. (*e*) Time taken to reach 'level 2'.

It appeared that full adaptation had not been reached, in the majority of cases, during the 40 min. test period. Accordingly, (*a*) is not a true measure of maximum sensitivity, nor is (*c*) a true indication of what it was chosen in order to measure: the increase in a

given individual's sensitivity brought about by D.A. Nevertheless, (*a*) and (*b*) may be regarded as measures of *extent* of D.A., (*d*) and (*e*) of *time* or *rate*, while (*c*) holds an intermediate position.

These measures were intercorrelated for the normal and neurotic groups taken separately, with the results noted in Table 5. It will be seen that for both groups (*a*) emerges as the best single measure, but that with the exception of (*c*) they are probably almost interchangeable. A fuller discussion of the intercorrelation of different measures is given by Cobb(6).

Table 5. *Intercorrelations* (*all positive*) *between measures of* D.A.

(i) Neurotic group: P.E.'s betwen 0·06 and 0·01

	(*a*)	(*b*)	(*c*)	(*d*)	(*e*)
(*a*)		0·83	0·95	0·82	0·86
(*b*)	0·83		0·73	0·94	0·79
(*c*)	0·95	0·73		0·70	0·83
(*d*)	0·82	0·94	0·70		0·84
(*e*)	0·86	0·79	0·83	0·84	
Totals	3·46	3·29	3·21	3·30	3·32

(ii) Normal group: P.E.'s between 0·10 and 0·02

	(*a*)	(*b*)	(*c*)	(*d*)	(*e*)
(*a*)		0·73	0·78	0·87	0·70
(*b*)	0·73		0·50	0·79	0·90
(*c*)	0·78	0·50		0·35	0·41
(*d*)	0·87	0·79	0·35		0·91
(*e*)	0·70	0·90	0·41	0·91	
Totals	3·08	2·92	2·04	2·92	2·92

V. Discussion

As stated in § II (2), choice of apparatus and technique in the measurement of D.A. must inevitably depend to some extent on the purpose of the investigation in hand. Nevertheless, it seems possible to distinguish fairly clearly between what may be termed 'necessary' and 'unnecessary' variations respectively.

With regard to time allowed for the actual test, much depends on whether it is desired to reach 'complete' adaptation. This in turn depends on whether foveal or peripheral adaptation is to be measured. If foveal only, then, as has been demonstrated by Hecht(13) and others, the process is complete within about 10 min., and there is no point in continuing beyond this time. On the other hand, it seems probable, at least at first sight, that peripheral dark adapted vision is of greater importance in the practical situation. It is often assumed that complete peripheral adaptation is reached in about 40–60 min., but the writer's own experience of 2 hr. D.A. sessions has suggested that sensitivity often seems still to be increasing at the end of that period. In fact, curves for the 6 hr. sessions described elsewhere(27) seemed to show a tendency to rise throughout that time, and Achmatov(1) even claims a continuous rise during a period of as much as 24 hr. It is not of course suggested that a test of D.A. must, to be valid, extend over a period of 24, 6, or even 2 hr., since logarithmic plotting has shown the later increases to be relatively slight. The important thing is to determine what degree of 'completeness' of D.A. may be accepted as good enough for different practical purposes, and as yet no opinion has been expressed on this point, except perhaps in so far as it is to some degree implicit in the choice of length of test period. In other words, it would be well to determine at what level, as it were, of D.A., individual differences are most significant. If, again, form perception, or even that of movement, should prove to be more significant than pure sensitivity to light, foveal adaptation would probably be sufficient. As things are at present, form and light discrimination are frequently confused, and if, as appears from results obtained by Helson & Fehrer(17), thresholds for form are 12–25 times those for light, the discrepancy is a serious one.

A further point regarding time, although it has wider implications, is the question of

preliminary adaptation. If extremes of daylight illumination are avoided it seems probable that the preliminary D.A. period mentioned in § II (2) can be eliminated. As to light adaptation or 'bleaching', it is perhaps best to set aside the possibility of general facilitation of subsequent D.A. by this means, and assume preliminary light adaptation as a necessary condition of uniformity, with the period and intensity such as to bring about the maximum possible change in both retinal and pupillary condition.

The above may be summed up by saying that given a sufficiently strong pre-adaptation light, and test conditions under which the subjects do not pass into the test room direct from extremes of daylight, not more than 5 min. preliminary light adaptation should be necessary, followed by as long a period of D.A. as practicable, not less than 40 min. if anything approximating to full peripheral adaptation is desired. This desideratum is assumed in what follows.

The necessity has often been urged for statement of the illumination value of the test light in absolute units. Ideally this is of course desirable, but the characteristics of reflecting surfaces are such that this measurement is extremely difficult to make with any but the finest instruments. Since illumination value varies with the size of the test field, standardization with respect to size is perhaps desirable, as well as in respect of retinal location. Probably, from the point of view of easy treatment of results, the test field should be so far displaced from the centre as to eliminate the characteristic 'dent' which appears in curves involving both foveal and peripheral adaptation, due to the supersession of the former by the latter (see Hecht *et al.* (14)). Optimal size, shape and location seem further to be affected by purely subjective conditions. My own view on this whole complex of variables is that it is probably better to devote one's energies rather to establishing adequate standards for one's own particular test conditions, than to strive to equate with those laid down by other investigators, particularly since agreement upon details, though desirable, is unlikely to be achieved. Interpretation in log units solves a multitude of difficulties, since so many log units divergence from a norm or standard has the same meaning whatever the conditions. Thus, for example, the careful work of Lythgoe & Phillips (22) has established a relation between monocular and binocular thresholds which amounts to a difference of about 0·15 log unit. On this point I should like to suggest that except for special purposes binocular observation is to be preferred, as experience has shown it to be more comfortable for the subject, at any rate with the more ordinary modes of occlusion of the idle eye.

This raises the important general question of how far it is possible to make the test situation strictly analogous to the practical one. This is of course a fundamental issue of psychological method, and much could be said in favour of either of two opposed views: (i) that no effort should be spared in attempting to ensure that the observer is responding to the full limit of his physiological capacity, and (ii) that undue strain and possible falsification of results should be avoided by keeping the conditions as 'natural' as possible. It is often hard to tell beforehand in what way a point of experimental technique will affect the observer. Thus, certain methods of testing D.A. require continuous alertness, so that the subject may respond as soon as a given stimulus is discernible. Some observers find this satisfactory, since they are able to feel that all the time they are 'doing something'. Some, on the other hand, find their attention is liable to wander, in which case periodic judgements or 'settings' of the adaptometer, are preferable. From both points of view, especially (i), practice should be given if at all possible.

As regards interpretation, two main points seem to emerge. The first relates to interpretation of actual adaptometer readings, the second to interpretation of differences. If rate of adaptation is the chief interest, the most useful measure for practical purposes is probably time taken to reach a given level of sensitivity (see § IV (5)). In such a case smoothing of curves is not so necessary as when maximum sensitivity during a given period is taken as the measure of D.A. In the latter case single anomalous readings may lead to false conclusions, and some method of eliminating or minimizing their effect seems essential. If sufficiently frequent readings have been taken, a method suggested in another connexion by Vincent(32) might be used. Here successive scores or readings are collected into groups of ten and averaged, and a curve based on these averages is drawn. My own practice has been somewhat similar, except that the groups have been smaller and made to overlap, i.e. averages taken for readings 1, 2, 3; 2, 3, 4; 3, 4, 5, etc. This procedure tends to distort the course of the curve in the early stages (as would Vincent's), but the discrepancies are small, and affect all cases about equally. Maximum sensitivity is probably a more significant single measure for most (though not all) practical purposes, but it is interesting that in certain cases, e.g. the experiments on vitamin intake reported in § IV (3), measures in terms of time seem to vary more regularly (see Table 2).

If for theoretical purposes it should be desired to study the individual's progress from initial (i.e. light-adapted) sensitivity to his own maximum or to a stated level, it would seem desirable either to ignore the reading obtained immediately on extinction of the pre-adaptation light, or to take the first reading say 1 min. afterwards, as in Matthey's method(24). The comparatively poor reliability of measure (*c*) as seen in Table 5 is no doubt due in part to the fact that with the apparatus available the subject had to change position before readings could begin, but very wide individual differences were also found in the subject's adaptability in settling down to the test situation.

Finally, interpretation of differences may vary in reliability as the result of a number of contributory factors. Craik & Vernon(8) quote 0·2 log unit as a range of variation which they evidently consider small if not negligible, but many investigators have put forward conclusions based on changes of about this magnitude, which have been taken to be real and significant. Again, Matthey(24) has proposed as a criterion of abnormality a curve of log $(M+3\sigma)$, where M stands for successive mean readings for her group of normals, and this curve proceeds for the greater part at a distance of little more than 0·4 log unit from the 'average' curve. If, as in this case, sufficient observations of a homogeneous character are available to allow of accurate statistical treatment, the difficulty of deciding on the magnitude of a 'real' difference does not arise. The standards of normality used by the present writer(35) depend mostly on differences of 1·0 log unit, and in assessing changes due to dietary or other conditions I should hesitate to place much reliance on smaller changes, except possibly at extremes of the scale, or when confirmed by repeated measurement.

VI. Conclusion and summary

Studies of dark-adaptation have shown wide divergences in respect of both technique and interpretation. Differences in the former respect are to some extent conditioned by the purpose of the investigation in hand, but it is suggested that an adequate test of D.A. should embody the following conditions: a duration sufficient to give at least ap-

proximately complete adaptation, for which, if peripheral vision is being studied, 40 min. is the absolute minimum; a preliminary period of light adaptation, sufficient to ensure uniform ocular conditions before D.A. begins; judgements at stated intervals rather than continuous attention to the test light. Account should also be taken of the size and retinal position of the test object, of whether it is viewed with one eye or with both, and whether judgements depend on form discrimination or purely on light sensitivity. Different writers have reported on all these factors, and others, and it should be possible to make satisfactory allowance for variations when comparisons are to be made.

In assessing adaptometer results a number of additional factors must be taken into consideration; of these some are constant and some alterable. Probable operative factors in the first class are chiefly age, intelligence, and certain eye conditions. The latter class includes the effects of practice, though these are not altogether calculable, and nutritional condition, though the validity of adaptometer tests as indicative of mild vitamin A deficiency has not been established beyond question.

Adaptometer results should be interpreted in terms of logarithms of threshold values. Curves thus obtained agree both with the bimolecular nature of the biochemical reaction believed to form the basis of dark-adapted vision and with the course of 'ability to see in the dark' as experienced by the subject. D.A. may be measured in terms of maximum sensitivity during a given test period, or of time taken to reach a stated threshold value. Choice of measure will be determined by the practical or theoretical implications of the problem in hand, and, at least if the former measure is used, calibration in standard illumination units is not absolutely essential. It is suggested that the individual investigator should as far as possible set up his own norms or standards, since a given number of log units displacement from a standard has the same meaning in whatever circumstances.

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EXPERIMENTS ON PARANORMAL GUESSING

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I. Introduction

The primary object of the present report is not to bring forward fresh evidence for the reality of telepathy and related phenomena, but rather to suggest a method of experimentation which may easily be reproduced by any psychologist wishing to test the matter for himself. Apart from the considerable body of earlier evidence, the recent experiments of Rhine and his collaborators(1), of Soal(2), of Tyrrell(3), and of Whately Carington(4), have put beyond question both the reality of the phenomenon and the possibility of its demonstration by experimental methods. Anecdotal reports of telepathy and of experimental successes under conditions of insufficient rigidity were met by a justifiable scepticism; the phenomena seemed intrinsically improbable since they did not fit into the framework of expectation based on the causal principles accepted by science. The intrinsic improbability remains, but the evidence for the reality of the phenomenon is now so overwhelming that scepticism can only be justified by ignorance of the experimental results. The task for experimental workers in the subject now is not to accumulate fresh evidence for telepathy and related phenomena, but rather to design experiments to test hypotheses about their nature and conditions. Plans of experimental research designed for the one purpose are generally unsatisfactory for the other. When the nature and conditions of these phenomena are fully known, their intrinsic improbability may appear less.

Experiments have generally taken the form of turning up the cards of a pack one by one and assessing the success of the subject by seeing how often the card he names is the one turned up. The pack used may be of ordinary playing cards, but Rhine has popularized a different kind of pack (Zener cards) consisting of twenty-five cards with five each of five different diagrams. The limitation of 'success' to successful naming of the card turned up is an obvious one, but it is now doubtful whether this is the best way of assessing results. It was first pointed out by Whately Carington (working with pictures on successive days) and amply confirmed by Soal (working with Zener cards) that subjects might not be succeeding in the task intended by the experimenter but in a different task which was, however, equally not to be explained by chance. The subject might be failing to name the card turned up, but succeeding in naming more often than could be explained by chance the next card to be turned up, or the card two ahead, or

he might be succeeding to some extent in any combination of these modes of guessing. This possibility indicates the necessity for a modification of experimental technique.

In experiments performed on several subjects, I formed the impression that failure was sometimes due to the subjects tending to guess on the card during one time of going through the pack while guessing one or two ahead at other times. It must be remembered that with a limited pack (i.e. with a fixed number of cards of each kind) a tendency to guess correctly one ahead will reduce the probability of right guessing on the card and vice versa. This difficulty would not exist with an unlimited pack such as those used by Soal in which the numbers of cards of each kind in any one pack are random. The rate of success (if there was any real success) was low and no positive conclusion could be drawn from these experiments, but the suspicion was sufficiently strong to make further research worth while.

II. Experiment 1, using heterogeneous system of packs of the same constitution as the Zener pack

The first attempt to overcome this difficulty was by the use of a heterogeneous set of packs. One ordinary Zener pack was used, a 'colour pack' was prepared with five of each of five different colours, and a 'number pack' with 1, 2, 3, 4 or 5 white circles on a black ground, again five of each in a pack of twenty-five. One card was drawn from each pack in turn and the experimenter said: 'Zener', 'colour' or 'number'. It was hoped, in this way, that the time interval between two guesses of the same kind and the interposition of two different kinds of guesses between each would eliminate the tendency to guess forward.

I found that the only subject available who seemed to be giving promising results was myself. I, therefore, acted as subject while another person (referred to as the 'agent') turned over the cards and wrote down both the card turned up and my guess after I had guessed. I was not informed of successes until one set of packs was completed. While the experiment was going on I lay on a sofa with muscles relaxed and eyes closed. It is generally stated (by Rhine(1)) that bodily relaxation favours the phenomenon, but it is not clear whether this opinion is based on an exact numerical comparison of series obtained under conditions of bodily tension and relaxation or whether it is merely an impression gained in the course of experimenting. My conative attitude was not that of trying to guess what card was turned up but rather that of taking no interest in the experiment and giving the first response that came into my head. I know of no experimental evidence that this is the most favourable conative attitude, but it seemed on theoretical grounds that it might be best. A comparison between this and the attitude of making an effort to guess the card must be a subject for future research. The experiments were performed in the evening and, since Rhine(1) states that alcohol favours the phenomenon, I usually drank half a pint of beer or cider at supper. The packs were, of course, thoroughly shuffled after each experiment.

The result of 600 guesses under these conditions with my wife acting as agent showed the following number of right guesses on the card: Zener, 43; colour, 34; number, 52.

The mean chance expectation of each was 40 and the standard deviation of each was 5·6. While the results for the Zener and colour cards was clearly insignificant, the excess of 12 right guesses on the number cards would be significant if this were an isolated experiment. It is, however, the selected best of three experiments, and if this

is taken into account, the high value of the deviation cannot with any conviction be said to be beyond what might be attributed to chance. The result, however, remains sufficiently suggestive to indicate the need for further research. None of the results showed any indication of guessing ahead, and it looked as if the heterogeneous system of packs were succeeding in its object.

It may be noted that even if the results proved conclusively that the number cards were giving better results than either of the other kinds of pack, this would not necessarily show that what was printed on the cards had any significance for success. There is experimental evidence (Rhine and Soal) that success is not affected by the appearance of the cards. It is likely that the explanation of the difference in success (if this is real) is the difference in what the subject has to guess. That is, the same result might have been found if the dots on the number cards had been replaced by figures or even by letters without affecting the result so long as the subject had to guess numbers.

III. Experiment 2, using the same method as Experiment 1 but with different agents

A further series of seventy-five guesses with each pack was made by the same subject with Dr B. P. Wiesner and Dr Mary Barton acting as agents. Success was no longer expected from the Zener or colour cards, but these were still retained in order to keep a heterogeneous system of packs. Again, the Zener and colour packs gave insignificant results. For the number pack, the results were as follows:

	Hits on the card turned up	Hits on the next card	Hits on the card after the next
Observed	12	24	6
Expected	15	14·4	13·8
Deviation	−3	+9·6	−7·8
S.E. of deviation	3·5	3·4	3·3

There is a rather remarkable deficiency of hits on two cards ahead which may or may not have been accidental; in view of results to be considered later (§ VI), it is very likely that this deficiency is not accidental. The most striking feature of the results, however, and the only one with which we are at present concerned, is the large excess of hits on the card following the one turned up. This is nearly three times its own standard deviation, but its significance is reduced by the consideration that it is the selected best of three comparisons. Even when this is taken into consideration, however, the probability of so large a deviation occurring by chance remains small ($P = 0{\cdot}015$ instead of $P = 0{\cdot}005$).

It is necessary, of course, to be aware of the possibility that if a sufficiently large number of the properties of a set of figures are examined, some will appear to be significant, and that one may be led to commit the fallacy of attributing significance to mere accidental oddities of the figures. For this reason little reliance can be placed on results of this order if there is no previous reason for expecting them. It might appear a priori that the ability to guess a card correctly before it is turned up is so unlikely that any apparent indication of its presence should be rejected on this ground. The result was, however, not unexpected, partly because there had been indications of such an ability in many of my previous experiments, but also on the much stronger ground that this result has been obtained by Soal with a frequency which leaves no possibility of doubt that it is not accidental.

Even with the a priori improbability removed by Soal's work, these results could not properly be claimed as providing additional evidence of weight for forward guessing. This was not, however, my concern at this time. I was anxious to know whether the use of a heterogeneous system of packs would remove the tendency to guess forward, and these results give sufficient indication for practical purposes that it does not. It was necessary, therefore, to try to overcome this difficulty by some other method.

IV. Experiment 3*a*, using regional scoring with playing cards

The obvious solution seemed to be to accept the fact that guessing might tend to result in hits more numerous than could be attributed to chance on other cards than the one actually turned up and to adopt some system by which hits were scored over a region of the cards. The region adopted was from two cards before the one turned up to two cards later than the one turned up. Comparison could then be made between the hits actually observed over this region of five and those expected by chance. Since the pair of guesses at each end is not the centre of a region of five, the first two and the last two guesses were ignored in the scoring.[1] This results in some loss of information but it is not enough to matter.

A serious difficulty arises in using Zener cards or any other type of pack similarly made up. There are only five kinds of cards in such packs, so the mean chance expectation of score by a regional scoring method is high, and the test would consequently be very insensitive. If this method of scoring is to be adopted, it is obviously desirable that a pack should be used with a larger number of different symbols than five.

It would be possible to construct such a pack, but the conditions required seemed to be ideally fulfilled by the ordinary pack of playing cards. The fact that the pack contains fifty-two different individuals all different from one another simplifies the calculation of chance expectation and also increases the sensitivity of the test. The only possible objection is that the task of choosing between fifty-two individuals instead of five might make the subject less successful. Apparently this was the case, but the close resemblance of the playing pack with the number pack, which in Exps. 1 and 2 had given the best results, gave reason for hoping that playing cards would prove satisfactory material.

The agent was again my wife, and the conditions of the experiment were in all respects the same as those of Exp. 1. The calculation of chances and of standard error are given in Appendix I. If the first two and the last two guesses are ignored, it is found that the mean chance expectation for each pack is 4·615 hits on one of the neighbouring five cards, and that the standard error of the deviation from expectation is $2{\cdot}043\sqrt{N}$ (where N is the number of packs used).

In this experiment, eleven sets of guesses through the pack were made. The scores (in order) were: 5, 9, 5, 7, 9, 3, 5, 5, 8, 5, 4. The total number of hits was 65. This is 14·2 in excess of chance expectation with standard error of 6·78. The deviation is greater than twice its standard error, so the odds against it occurring by chance are greater than 20 to 1.

If the purpose of this experiment had been to add new evidence for the existence of telepathy, it would have been necessary to continue with it until the odds against chance

[1] This is the method of scoring actually adopted, but it is arguable that it would have been simpler to have retained the two end pairs of guesses. For experiments with playing cards, the mean chance expected score would then be 4·885, and the standard error of the total score would be approximately $2{\cdot}15\sqrt{N}$.

were much heavier than this. As I have already indicated, however, I consider that this has already been sufficiently done by others, and the above result was sufficient for my purpose at this stage of testing the suitability of this simple and easily repeatable form of experiment. At this stage, therefore, this set of experiments (3*a*) was brought to an end. Later experiments of the same type furnish additional evidence as to the possibility of the results being attributable to chance.

V. Control check of the guesses in Experiment 3*a*

The significance of the results in the above experiment obviously depends on the correctness of the calculation of chance expectations. These calculations (Appendix I) are, I think, straightforward and correct. It seemed worth while, however, to strengthen conviction of their correctness by rescoring all the guesses of Exp. 3*a* against a new set of shufflings of the pack. After the completion of this experiment, therefore, a control check was made in the following way. The pack was shuffled and the guesses of pack 1 in Exp. 3*a* were scored against this new pack in exactly the same manner as the original scoring was made. The pack was then reshuffled and scored against the second series of guesses, and so on. The scores obtained in this way were: 3, 9, 3, 3, 4, 3, 6, 5, 4, 6, 3—total 49.

If the mathematical assumptions of Appendix I were correct, this control set of scores should show a mean and standard deviation not differing materially from the theoretical expectation of 4·61 for the mean and 2·04 for the standard deviation of the pack scores. The mean obtained was 4·45, which is very near the chance expectation of 4·61 and very much less than the experimental mean of 5·91. The standard deviation obtained from the control check was 1·92, which also is very near the expected value of 2·04. This control check, therefore, gives no reason for doubting the correctness of the calculations of mean chance expectation and deviation for this form of experiment.

VI. Distribution of hits in Experiment 3

It is natural to ask whether the successful results of Exp. 3*a* were due to hits distributed equally over all the five cards in the region examined or whether there was a marked tendency to guess correctly on one of these cards. Also, how wide was the region over which successful guessing was taking place? It must be remembered that some of the answers to these questions may be peculiar to the individual subject and to the particular conditions (such as speed) of the experiment.

In order to give a clearer picture of the range over which hits took place, hits were counted on the cards three and four behind the card turned up and also those on the cards three and four ahead. Results from Exp. 3*b* (nine packs) obtained under the same conditions as those of 3*a* are also shown. The mean chance expectation of hits in each position is $48/52 = 0{\cdot}923$.[1]

The result is shown in Table 1, and (with the addition of displacements of +5 and +6) in Fig. 1.

[1] In order to make the mean chance expectations the same for hits in the −4, −3, +3, and +4 cells as in the others, hits on the card three behind were counted also for the second guess from the end, those for the card four behind for both of the last two guesses, and so on. Thus the number of guesses used was 48 per pack in these as in the other cells.

It will be seen that over most of the range examined, the number of hits does not appreciably differ from chance expectation. For example, the number of times in which the subject succeeds in his aim of guessing the card turned up (displacement 0) is not different from what would be expected if the guesses were merely random. When, however, we turn to the +2 column which shows the number of times the guess was identical with the card turned up the time after next, we see a considerable excess over chance expectation; the number is, in fact, nearly double that expected. There is also a striking deficiency in the number of hits on the card three before the card turned up.

Even those willing to accept the possibility of telepathy may feel unwilling to consider the possibility of any real tendency to guess a card which has not yet been turned up and which is, therefore, not, at the time of guessing, known even to the agent. Still more may we reasonably feel repelled by the suggestion that there is something at work which leads the subject to avoid guessing those cards which were turned up somewhat before the card at which the guess is aimed. If there were no previous reason for

Table 1. *Displacements of hits in Experiments* 3*a and* 3*b*

Exp.	*N*	−4	−3	−2	−1	0	+1	+2	+3	+4
3*a*	11	8	5	13	12	14	11	17	10	11
3*b*	9	10	5	4	9	4	15	18	6	7
Total	20	18	10	17	21	18	26	35	16	18
Expected		18·5	18·5	18·5	18·5	18·5	18·5	18·5	18·5	18·5
Deviation		−0·5	−8·5	−1·5	+3·5	−0·5	+7·5	+16·5	−2·5	−0·5

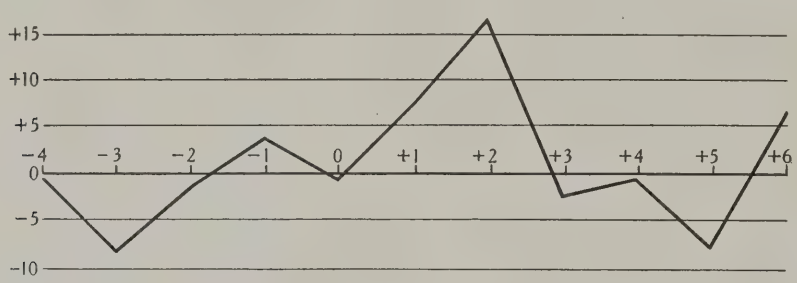

Fig. 1. Displacement of hits in Exps. 3*a* and 3*b*.

expecting effects of this kind, they might well be dismissed as mere oddities of the figures for which it would be misleading to attempt to determine the significance. Both effects have, however, already appeared in Soal's experiments with Zener cards (2). He has found subjects who consistently guess two ahead and who show minima a few cards before the one aimed at. These results have been obtained with a much larger number of guesses than I have made and are of unquestionable significance.

There are three questions of a statistical order which arise in connexion with Table 1: (1) Are the results as a whole consistent with random deviations from the expected values, and if not, (2) is the excess of hits at +2 significant, and (3) is the deficiency of hits at −3 significant? The detailed calculations for answering these questions are given in Appendix II.

The answer to the first questions is that the results as a whole are not consistent with the hypothesis that the deviations from the expected values are random ones. The odds are more than 100 to 1 against deviations from chance expectation as large as these

occurring by chance. There is thus sufficient evidence that there is some capacity on the part of the subject to direct the guesses according to the actual cards, although obviously this capacity works in an odd way.

The answer to the second question is equally definite. If we could properly consider the +2 cell in isolation, the odds against such a deviation occurring by chance would be about 5000 to 1. If we take into account (as we should) that this is the selected best of nine values, the odds against it occurring by chance remain more than 500 to 1, which is comfortably within the limits for significance.

The third question is more difficult. P turns out to be 0·06, which is about the conventional limit for significance. Again, however, we must take into account the fact that this is the selected lowest of nine values and the likelihood of a deviation as great as this occurring at least once in nine times is $(1-0{\cdot}94^9)$ which is about 0·6. This is probably an underestimate of the significance. It is clear, at any rate, that the figure so far obtained cannot be regarded as sufficient proof for the minimum at this point, although the previous evidence obtained by Soal makes it very probable that it is a real one.

The finding of an apparent tendency to score below chance at −3 suggest the possibility that a similar minimum might be found in the other direction. Hits on +5 and +6 were therefore also counted with the following result:

	+5	+6
3*a*	3	15
3*b*	7	9
Total	10	24
Expected	18·1	17·7
Deviation	−8·1	+6·3

There seems, therefore, to be some indication of a tendency to guess below chance before the point of maximum scoring as well as behind it. This indication must be accepted with even more caution than the other, since I know of no independent evidence for it. It may, however, be noted as a possibility to be confirmed or refuted by later evidence.

There is a point of theoretical interest which may profitably be considered here. When the correctness of a guess is determined by knowledge in some other person's mind not communicated by any normal means, the process is commonly said to be 'telepathy'. If a correct guess is obtained without corresponding knowledge in anyone's mind, the process is called 'clairvoyance'. If it is of an event which has not yet happened, it is called 'precognition'. Hits obtained on a card not yet turned up cannot, if we use these traditional terms, be considered to be simple telepathy. It might be called either precognition of a future knowledge in the agent's mind or clairvoyance of the card next but one in the pack. There is, however, another possible way of regarding the matter; these may not be different types of process. There may be only one process which does not have the exact restriction to present time and to present sensory stimulation that is characteristic of the process of obtaining knowledge by ordinary sense perception. The process may be influenced in some way by knowledge in another person's mind, but this may not be an essential condition of its activity. There is already evidence of this in Rhine's work who finds his subjects scoring in 'pure clairvoyance' experiments with the same level of success as in 'pure telepathy' experiments (1).

It may thus be that the terms 'telepathy', 'clairvoyance', 'precognition' are misleading as implying a difference of process when they refer only to one type of process which can be found under different experimental conditions. If this is so, it would seem better to use one name for the process, instead of three. Rhine has already used the term 'extra-sensory perception' in this sense. This term is open, I think, to the very grave objection that it implies that the process is a special kind of perception. There seems to be no evidence for this view and much against it. In any case, it seems premature now to commit ourselves to a term which implies a theory which may be false and which may be misleading as a guide to research. It seems better, therefore, that we should use a new term committing ourselves to no theory, and I suggest the adoption of a term suggested by Dr Wiesner, and that we should call all these effects the 'psi-phenomenon'.

VII. The problem of optimal conditions for scoring: attempt at Experiment 4

Three conditions were adopted in these experiments because it was supposed that they would be favourable to the psi-phenomenon. These were: muscular relaxation, absence of effort to guess, and a glass of beer or cider at the meal before the experiment. It was not known with certainty whether these conditions were necessary or even favourable to success. The first and second conditions have been reported as favourable by other workers. The principal reason, however, for adopting all three conditions was that they seemed likely to be optimal conditions on theoretical grounds if a general view of the matter adopted as a result of discussion between Dr Wiesner and myself were correct.

It will perhaps be convenient here to indicate briefly what is this theoretical view which determined expectations in these experiments. It seemed to us that the hypothesis with which one might start is that success in paranormal guessing is not, as supposed by F. W. H. Myers(6), a new mental capacity towards which man is evolving but rather a primitive one belonging to a lower stage of organic evolution than that of sensory cognition. If this is the case, we may regard it as a capacity normally inhibited by sensory perception and most likely to be detected under conditions unfavourable to sensory cognition. As suggested by Bergson(7) it may be expected to be found optimally under conditions in which there is a voluntary or involuntary inattention to the demands of practical life. Muscular relaxation creates such an inattention to the demands of practical life; so also does alcohol. Mental effort towards correct guessing brings into play the mental set effective in ordinary sensory perception, so the absence of such effort may be expected to be favourable to the psi process.

This speculation suggests a possibility as to successful subjects which may prove a useful guide to experimental research. It may be that the subject showing success in psi guessing is simply one who either is spontaneously 'inattentive to life' or who has learned voluntarily to adopt that attitude. While the spontaneously successful subjects (and perhaps always the most successful) may be those who are to some extent dissociated, such techniques as Jacobson's method of progressive relaxation(8) make possible for other subjects the voluntary adoption of the required attitude of inattention. It may be, therefore, that by suitable training the number of successful subjects could be enormously increased, and so the principal obstacle to a study of psi effects by any experimentalist interested in them would be overcome.

In order to try to discover whether the above expectations as to optimal conditions were fulfilled, the obvious course is to make a series of experiments with all possible combinations of each of these conditions and their opposites. In experiments without muscular relaxation, for example, the subject can stand (with eyes still closed). Experiments in which (as in Exp. 3) the subject was simply giving the first card name that came into his head can be alternated with others in which he makes an effort to guess what card has been turned up.

These experiments need not be described in detail since they were abandoned after the sixth pack because it seemed clear that they were giving nothing but chance results. The scores were 4, 7, 3, 4, 2, 6 (mean = 4·3).

A drop in the rate of scoring was, of course, to be expected, since this might simply be the result of the fact that optimal conditions were no longer being employed except in one (the first) of the packs. Even this, however, showed a low score, and it was suspected that there might be a more general reason for the decline of scoring. This was confirmed by the fact that when the conditions of Exp. 3 were resumed for two further evenings, chance scores were again obtained. These are the first three packs of Exp. 3*b* reported in the next section.

Since the failure of successful subjects to go on scoring has been one of the difficulties of experimenting in this field (as found by Rhine(1)), it may be worth while to consider possible causes for the failure at this stage. It was certainly not boredom on the part of the subject with the long continuance of the experiment. I started the experiments casually, more as a diversion than as a serious research project, but became keenly interested in them as positive results began to accumulate. At this stage, I was occupying all my spare time up to the moment of actual experimentation with writing up earlier results and performing calculations in connexion with the experiment. I am inclined to attribute the failure to this heightening of interest. If absence of effort to guess right is really favourable to success, it is an attitude difficult to maintain when interest in the results becomes intensified. Even when I was trying to adopt the attitude of responding with the first card name that came into my head rather than trying to get the card right, I may not have been successful in attaining this attitude since it is incompatible with keen interest in the results. I allowed a day to pass without experimenting and, on the following evening (of a day in which my spare time was occupied with other work and not with these experiments), three sets of guesses were again made (the second three of Exp. 3*b*). The results were now positive with an average score of 7.

The attempt to determine optimal conditions was temporarily abandoned, since it seemed desirable to see whether a method could not first be devised of so modifying the experiment as to get a higher average score. The average score for Exp. 3*a* was 6. Since the mean chance expectation was 4·6, this means that any capacity of the subject to guess right was only effective to the extent of bringing on an average $1\frac{1}{2}$ extra hits within the investigated range of five. In Exp. 1, on the other hand, an average deviation of about the same amount was found on each pack, but here the packs were only of twenty-five guesses instead of the forty-eight of Exp. 3, and this excess was on a single card instead of a region of five. It appeared, therefore, that the system of five cards used in Exp. 1 was, for some reason, considerably more effective in producing positive results than the pack of playing cards.

VIII. Comparison of scoring by number alone with scoring by cards (Experiments 5 and 3*b*)

It seemed possible that the fifty-two separate cards of the ordinary playing pack might impose a problem more difficult for the subject than correct guessing from a smaller number of individuals. Conditions more closely resembling those of Exp. 1 might be obtained by ignoring the suits of the cards and so reducing the pack to one of thirteen individuals repeated four times in each pack instead of fifty-two individuals.

The theory of scoring in this case is given in Appendix III. The simplest method seems to be to mark the number of guesses which show at least one hit on the cards turned up within the region of five (counting this as one success whether there is one hit or more within this region). The mean chance expectation per pack is then 16·38, and the standard error of the total is $3{\cdot}285\sqrt{N}$, where N is the number of packs.

This method of scoring may, of course, be used on results already obtained by the method of Exp. 3 by simply ignoring the suit in the marking. Casual examination of the results of Exp. 3*a* suggested that the subject had often succeeded in getting the number of the card right when the suit was wrong. Scoring by the new method showed

Table 2

	Exp. 3*b* (guessing whole card)		Exp. 5 (guessing numbers only)	
	Scores	Deviation from expectation	Scores	Deviation from expectation
	9, 0, 3	−1·8	14, 15, 14	+6·1
	5, 8, 8	+7·2	11, 18, 18	−2·1
	7, 5, 5	+3·2	15, 14, 14	−6·1
Totals	50	+8·6	133	−14·3

that this appearance of higher success when suit was ignored in the scoring was altogether illusory. The first six packs of Exp. 3*a* were scored by this method, and the result was that the absolute amount of the deviation was just about the same as before. Since, however, the standard error was larger by the new method than by the old, the new method of scoring gave a less sensitive test. Since the deviation remains the same in both tests, it appears that successful guessing (i.e. guessing above mean chance expectation) always gave both number and suit and there was no additional tendency to guess the number right without the suit. This was not an unexpected result, since I pointed out several years ago that this is also to be noticed in the results of Coover's card guessing experiments (5).

Even though rescoring by the new method of results, in which the subject guessed both number and suit, gave no better result than the old way of scoring, it remained possible that better results might be obtained if the subject were given the simpler task of guessing number only.[1] A new series of experiments was, therefore, started in which on alternate evenings guesses were made of the numbers only of three packs and of the number and suit of three packs. These are referred to as Exps. 5 and 3*b* respectively. The conditions as to relaxation, absence of effort, etc., were the same as in Exps. 1–3*a*. The results (in order of time) were as shown in Table 2.

[1] Here and elsewhere, I am using the word 'number' to include 'Jack', 'Queen' and 'King' as well as the numbers 1–10. The court cards were always retained in the packs.

It did not seem worth while to continue further with this experiment. The lowness of the scoring by the second method was unexpected, but I was not interested in discovering whether this was significant. The only point of practical importance was whether the method of Exp. 5 would produce a higher rate of scoring than that of Exp. 3. In view of the fact that the standard deviation was rather more than half as great again as that obtained in guessing the whole card, the guessing of the number alone would have nothing to recommend it unless the rate of scoring were considerably higher by this method. It was sufficiently clear that it did not give the required improvement in scoring so the method of guessing the card alone was given up and not used again.

I am doubtful of the value of introspection as evidence as to causes in experiments of this kind, but, for what it is worth, I have the impression that the reason for the failure of this experiment was the relatively uninteresting nature of the task. Even to one who plays cards very little, the cards of a playing pack are strongly individualized and the question of whether a card is the nine of clubs is more interesting than the question of whether it is a nine, just as the question of whether someone is the individual John Smith is more interesting than whether his name is Smith. If this is the explanation, it may be an individual peculiarity of myself; others might find the numbers as strongly individualized as I find the cards. In earlier experiments it has been suggested strongly that the nature of the material guessed is by no means indifferent to results, and that I showed no signs of ability to guess Zener cards which have been successfully used by Rhine's subjects even when I was guessing successfully with number cards (containing only the more individualized lower numbers). This suggests that it may be worth while in experiments of this sort to use different material for different subjects. For the investigation of some questions, such as those of guessing ahead, there are, however, distinct advantages in having as many different cards as possible, and I think that playing cards are therefore to be recommended unless it may chance to be found that they are not giving good results with a particular subject.

APPENDIX I. THEORY OF REGIONAL SCORING OF GUESSES ON PACKS OF PLAYING CARDS

The first two and last two guesses were ignored and a region of five centred about the card corresponding to the guess was used.

The probability of a guess being a hit on any one card is 1/52.

Therefore, the probability of getting one hit on a card within the region of five is 5/52.

There are 48 guesses in each pack so the mean chance expectation of hits per pack is $48 \times 5/52 = 4{\cdot}615$.

The standard deviation of hits is given by the formula for a logarithmic series: $\sigma = \sqrt{(npq)}$, where n is the number of trials, p is the chance of success in any one trial, and q is the chance of failure in any one trial.

$n = 48N$ if N is the number of packs, $p = 5/52$, and $q = 47/52$.

So the standard deviation for the total score is $1/52 \times \sqrt{(5 \times 47 \times 48N)} = 2{\cdot}043\sqrt{N}$.

To make clear the method of scoring, I add the scoring of one pack (the first of Exp. 3*a*). The guesses in square brackets are those ignored for the purpose of scoring, the figures in brackets after guesses show the positions of hits (Table 3).

For the convenience of those who may wish to perform experiments of this sort for themselves, I add a table (Table 4) showing expected scores and standard deviations for various numbers of packs and also the minimum total scores necessary for significance at the minimum level of $P = 0{\cdot}05$ and also at the higher level of $P = 0{\cdot}01$.

Table 3. *Example of scoring*

Card	Guess	Card	Guess	Card	Guess	Card	Guess
1S	[2H]	QH	3S	3H	9C	2H	6C
JC	[2C]	4D	QS	KC	KH	3S	8H
1D	7D	1H	10C	5C	JD	KH	9D
5S	JC (−2)	5H	KS	6D	5H	4H	6D
QS	QS (0)	7H	5D	5D	7S	8S	3D (+2)
KD	8H	10D	4H	7C	9C	6H	2H
10C	2D	6S	3C	1C	10S	3D	10C
4S	3C	9S	8H	2C	QH	3C	QH
QC	9D (+2)	6C	9D	9H	7D	2D	KS (+2)
10S	2S	8D	2C	JS	2H	JH	JD
9D	QD	8H	6H	8C	5D	KS	9C
JD	1H	7S	JS	9C	3H	7D	[2S]
10H	6C	2S	10S	4C	4S	QD	[6H]

Total score = 5.

Table 4

No. of packs	Mean chance expectation of hits	Standard deviation of total	Minimum score for significance $P=0.05$	Minimum score for significance $P=0.01$	No. of packs	Mean chance expectation of hits	Standard deviation of total	Minimum score for significance $P=0.05$	Minimum score for significance $P=0.01$
1	4·6	2·04	9	10	12	55·4	7·07	70	74
2	9·2	2·89	15	17	13	60·0	7·37	75	79
3	13·8	3·54	21	23	14	64·6	7·65	80	85
4	18·5	4·09	27	30	15	69·3	7·91	85	90
5	23·1	4·57	33	35	20	92·3	9·14	111	116
6	27·7	5·00	38	41	25	115·4	10·21	136	142
7	32·3	5·41	43	47	30	138·4	11·19	161	168
8	36·9	5·78	49	52	50	230·7	14·45	260	268
9	41·5	6·13	54	58	75	346·1	17·70	381	392
10	46·1	6·46	59	63	100	461·5	20·43	502	515
11	50·8	6·78	65	69					

Appendix II. Calculations of significance for Table 1

(1) In Table 5 is shown the significance of deviation from expectation of Table 1.

Table 5

	−4	−3	−2	−1	0	+1	+2	+3	+4	All others	Total
Observed	18	10	17	21	18	26	35	16	18	781	960
Expected	18·5	18·5	18·5	18·5	18·5	18·5	18·5	18·5	18·5	793·5	960
Deviation	−0·5	−8·5	−1·5	+2·5	−0·5	+7·5	+16·5	−2·5	−0·5	−12·5	—
Deviation²	0·25	72·25	2·25	6·25	0·25	56·25	272·25	6·25	0·25	156·25	—
$\frac{\text{Deviation}^2}{\text{Expected}}$	0·01	3·91	0·12	0·34	0·01	3·04	14·72	0·34	0·01	0·20	22·70

$\chi^2 = 22{\cdot}70$ with 9 degrees of freedom, so $P = 0{\cdot}007$.

(2) For determining the significance of the maximum at +2, χ^2 may be calculated with one degree of freedom by making a comparison between the number of hits at +2 with those in all other positions, i.e. the difference between those at 6 and the total number of hits which must be one for every guess, 48 × 20 in all.

	Hits on +2	Hits elsewhere	Total
Observed	35	925	960
Expected	18·5	941·5	960
Deviation	+16·5	−16·5	—
(Deviation −0·5)²	256	256	—

$\chi^2 = 256/18{\cdot}5 + 256/941{\cdot}5 = 13{\cdot}84 + 0{\cdot}27 = 14{\cdot}11$. $P = 0{\cdot}0002$.

(3) For the minimum at −3.

	Hits on −3	Hits elsewhere	Total
Observed	10	950	960
Expected	18·5	941·5	960
Deviation	−8·5	+8·5	—
(Deviation −0·5)²	64	64	—

$\chi^2 = 3{\cdot}53$. $P = 0{\cdot}06$.

APPENDIX III. THEORY OF REGIONAL SCORING WITH A PACK OF PLAYING CARDS WHEN NUMBER ALONE IS CALLED

The first two and the last two guesses were ignored. Score is number of guesses giving at least one hit (not the total number of hits, which may be larger).

The probability of failing to score a hit by any one guess on any one card is 48/52. If in the region of five there is failure to get a hit on any one card, the probability of failing to score a hit on the next card is 47/51. If there is failure on any two cards, the probability of failure on the next is 46/50, and so on. The probability of failing to get a hit on all five cards of the region used for scoring is, therefore, $48/52 \times 47/51 \times 46/50 \times 45/49 \times 44/48 = 0{\cdot}659$. If the score is taken as the total number of guesses which have at least one hit on their region of five, the probability of scoring on any one guess is $(1 - 0{\cdot}659) = 0{\cdot}341$. Mean chance expectation for each pack is $48 \times 0{\cdot}341 = 16{\cdot}37$.

The standard deviation of the total score for N packs is

$$\sqrt{(0{\cdot}659 \times 0{\cdot}341 \times 48N)} = \sqrt{(10{\cdot}7878N)} = 3{\cdot}285\sqrt{N}.$$

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IS THERE AN INSTINCT OF POSSESSION?[1]

By LÉON LITWINSKI

I. *The concept of instinct and the elements upon which it is based* (pp. 28–29). II. *The concept of the 'instrument' and that of the 'implement'; utilization and possession* (pp. 29–30). III. *Attention during suspended relaxation and patient attention* (pp. 30–32). IV. *The important problem of the origin of the instinct of possession* (pp. 32–34). V. *The problem of the instinct of possession in the child* (pp. 34–36). VI. *The so-called instinct of possession in man and animals* (pp. 36–39).

I. The concept of instinct and the elements upon which it is based

Science tries to free itself from ideas which, though commonly accepted, tend to confusion when badly defined. Should the concept of instinct, for instance, be for this reason abandoned, at least provisionally—a concept which is so readily employed when one is confronted with something mysterious or obscure? It must be admitted that the ordinary man as well as the scholar needs some term to describe a natural tendency which moves the individual to perform certain spontaneous acts. One calls these acts impulsive. They are simultaneously unchangeable and exact, irresistible and invariable, unconscious and innate. Ed. Claparède defines instinctive behaviour as "an act adapted and accomplished without practice, in a uniform manner, by all individuals of the same species, without knowledge of its aim nor of the relation which exists between this aim and the means employed to attain it". But if one analyses a definition based on the aforesaid elements, one can unfortunately consider it as provisional only, for the elements of which it is composed themselves need a clearer definition. It is difficult to try and elucidate something obscure by something else equally indefinite.

Let us take the concept of 'innateness' in certain organic dispositions. S. Guillaume, in his *Psychologie Animale*, stresses that it is always very difficult to prove that what one observes is independent of the influence of experience and of practice, and consequently to distinguish habit from instinct. A whole line of contemporary criticism disputes the validity even of the concept of instinct. This question must not be confused with that of the origin of hereditary dispositions in the history of the species; it is a question of those which are to-day found in the individual.

As to the unconsciousness of instinct, S. Guillaume admits that human psychology has much progress to make in the sphere of the varieties of behaviour which correspond to the various modes of consciousness, concentrated or diffuse, synthetic or analytic, personal or impersonal, etc., before one can risk interpreting the behaviour of animals in terms of consciousness.

If the concept of instinct is still used in the realm of science with more or less reserve, because of a certain vagueness which surrounds the concepts on which it must be based, on what does one rely in declaring—as one generally does without much thought—that possession is an instinct?

The lack of precision in the concept itself of instinct is doubtless an encouragement in this respect, a reason for the ease with which one uses the term instinct. Nevertheless,

[1] Translated from the French, 'Y-a-t'il un instinct de propriété?' by M Léon Litwinski, formerly Ministre Plénipotentiaire in Brussels, now resident in Portugal.

there is in addition, notably, the fact of the universality and the ancientness of what one readily calls the love of possession. It seems that the verbs 'to love' and 'to possess' have from time immemorial been conjugated together. Now, according to current ideas, the old established and the instinctive are completely unified.

II. THE CONCEPT OF THE 'INSTRUMENT' AND THAT OF THE 'IMPLEMENT'; UTILIZATION AND POSSESSION

That is not all. There is, besides, the lack of precision as to what constitutes the object of possession and also as to the possessive tie. The concept of the 'instrument' particularly is confused with that of the 'implement' in the matter of possession. The object by itself is nothing. Only in so far as it becomes instrumental from the point of view of behaviour does it acquire a meaning and a function. Now if it is a question of material objects, furniture, for instance, the object must be not only material but in addition external to ourselves. From this point of view everything which is an implement is at the same time an instrument, but not vice versa. Thus a finger-nail, which may be a means, an instrument, is not an object of possession; but a penknife is, because it is also an implement, a tool. But it is futile to try and take possession of our finger-nails. They are ours by force of circumstances. They are instruments but not implements. The animal uses its claws for defence, to feed itself, etc., just as man uses his legs for walking and his hands for working. But the claws, the hands or the legs are not objects of possession. Although Thiers (*De la Propriété*, 1849) considers the organs in question as the primary form of property, this seems to us ambiguous. It is ambiguous, as I have said elsewhere (*En Vue d'une Nouvelle Théorie Psychologique de la Propriété*), because the means which we cannot choose for ourselves are not instruments which can become objects of possession. According to Lévy-Bruhl, the various kinds of things which belong to primitive man, his 'belongings', form an integral part of his person; this applies not only to his secretions and excretions, but also to the trace of his footsteps, the remains of his food, the products of his work, the implements he uses. As we see, all the objects mentioned are external to the man who possesses them. To be able to create and maintain possession over detachable objects—that is, objects which need not be our property although in fact they are so—is a way of making ourselves entirely and to some extent consciously masters of nature, and not subject to it. Man is essentially a possessor.

The possession of claws and teeth certainly implies the mastery of them, but also enslavement by them. Possession of a rake or a knife implies also the mastery of them, but not necessarily enslavement. The possessor of implements has an enormous advantage, by comparison with the possessor of organs, over simple instrumental means, since he can liberate himself from them and eventually increase their number, or change their quality. It is the first great difference, from the point of view of instrumental activity, between man and animals, that the latter merely uses its organs, while the former is capable of using, in addition, and on a scale unknown elsewhere, implements and external objects in general, mobile or immobile, animate or inanimate. Thus the children of the peasant, as Balzac has pointed out, "are his capital, and instruments of his prosperity", until society or the state snatches them from him. Speaking of this set of implements, one must not forget the existence of a vast set of intellectual implements, particularly of the use of language which is peculiar to man.

If the animal is equipped for life by nature, man takes pride in so being by means of his powerful imagination. The animal develops its senses, while man decreases their efficiency, often to a dangerous degree, by the use or rather the lack of use he makes of them. Thus the power of sight in the animal is reinforced by its highly developed sense of smell, whilst man reinforces it by the use of spectacles, taking care besides to consult the oculist periodically in order to procure a stronger pair. There are circumstances in which both the animal and the man make use of speed. If it is a question of utilizing his own organs for this purpose, man resorts to the bicycle, for instance; but it often happens that the animal runs faster than the man can pedal. In short, the animal lacks the imagination to possess, but the psychologist sometimes has imagination to excess. He readily attributes to the animal possessive behaviour which without doubt still requires exact verification.

The simple and easily observed fact, that the animal in general does not encumber itself nor identify itself with the implements surrounding it, is already sufficiently revealing to indicate that the animal does not possess in the material sense of the word. Because someone has observed some day and somewhere an isolated case of a monkey which on occasion used a stick to pull objects nearer, or a stone to throw, or even to crack nuts because of the feebleness of its teeth, one cannot therefore declare the animal world to be equipped with material implements. But even if it were so, the fact of using these objects, of having the skill and technique to employ them, is not equivalent, one must hasten to add, to a possessive tie. This fact can at most become a basis for possession.

The animal has only a sensory awareness of objects, while man has in addition the awareness of his own existence, that is to say, the awareness of being aware, the sentiment of the 'self', of personal identity. Thus he attaches himself or detaches himself from objects, not only in accordance with the present interest of these objects, but also in accordance with their significance with regard to his capacity for the extension of vision and imagination in time, that is to say, beyond the present. The animal, on the contrary, seems to be clearly aware of the limitations of its means. At one moment it attacks, at another it flees, in accordance with its strength and skill. It is rare that it becomes the victim of its own rashness, pride or greed, as is the case with man who tends to imagine too much in view of his fundamental weakness.

In short, to make use of is not to possess.

III. Attention during suspended relaxation and patient attention

If to possess means the power of becoming tied to an object, as we have elsewhere submitted, through an attitude of relaxed and suspended attention, the animal does not possess in the psychological sense of the word.

In order to make this point clearer we must introduce a parenthesis, and insist on the fundamental difference between the attention which is shown by a possessor and the attention that one observes in an animal, which merely expresses the power of waiting patiently. We shall do it in conjunction with our concept of the possessive tie, on which subject I have already written elsewhere (*Qu'est-ce que la Propriété?* (Paris, 1913) and *En Vue d'une Nouvelle Théorie Psychologique de la Propriété* (1941)).

Let us take the case of the spider, observed by J. H. Fabre (*Souvenirs Entomologiques*).

It sometimes watches its prey for days even. It preserves a state of attention which is almost one of relaxation, so great is the spider's patience. This attention is, nevertheless, uninterrupted. It is a process which slowly unfolds. Passionately devoted to hunting, says Fabre, as long as it has no fixed abode the spider prefers to lurk in ambush and await its prey. Every day at the hottest time spiders take up good positions, ready to attack by springing out of their hiding places. "With heads protruding, glassy eyes glaring fixedly, legs drawn up for the spring, it waits for hours, motionless and soaked in sunlight." Very rarely is the prey lost, provided that it passes at a convenient distance within the radius of the hunter's spring. But if the prey is at some distance, the spider takes no notice of it. It disdainfully lets its prey wander on. To make its attempt, it must be certain of success. Hidden behind the wall, it sees the prey approach, watches it and, when it is within reach, suddenly springs on it. But if the prey, Fabre adds, does not come to-day, it will come to-morrow or the day after. The opportunity will come in the end. Then will be the moment to spring. Until then: "Impeturbable watchfulness. One eats when one can, but one eats in the end." Undismayed by these long-drawn-out proceedings, the spider then awaits the arrival of its prey, sometimes for days on end in prolonged abstinence. "It has an accommodating stomach, now well gorged with food, then empty indefinitely. All these voracious eaters are the same, they devour to excess to-day, foreseeing the want of to-morrow."

Foreseeing? Is the spider then capable of foresight? Let us rather say that everything happens as if it were. It is a question of instinct and not of reflexion. The movements of the spider are automatic, irresistible, invariable. It is the slave of them. It has no hesitation or adaptability. This only means that the things indispensable in one way or another to the life of the spider are acquired during the long existence of the species.

This patient waiting is in no way the attention of foresight, that is to say, attention with relaxation. It is, rather the psychological substitute, for those who have no provisions or stocks, nor reserves to satisfy their periodic needs. To be patient, enduring, to be able to deprive oneself, is perhaps a substitute in lieu of foresight—but it is certainly a genuine ability. There are human beings who specialize in these qualities, but the animals have them of necessity. This fact explains the enormous progress of mankind. Is he the happier for it? Not always. A Polish proverb says: "Gay because destitute." Possession creates cares, burdens, risks. The Americans who lead the way in material progress try to simplify the existence of civilized man by ridding him of certain of the anxieties of the possessor. In seeking a new formula for this, one no longer gives oneself the trouble of sending handkerchiefs to the laundry; they are immediately replaced. It is more economical and at the same time simpler. It is a notable example of progress. Not to possess too much will one day be the criterion of progress, of independence and of liberty.

Without seeking to introduce animal psychology here, it is the place to insist on the point that in the patient attention of the unsatisfied spider we are dealing with a kind of animal mechanism under permanent tension; whilst in the case of the peaceful attention of a happy possessor there is no question of tension without a moment of respite, but of a state of awareness which functions, ceases, to be resumed later, and is susceptible to alternation at will. It lasts as long as the subject maintains his interest in the object through force of the idea which ties him to this object. Now to possess an object without a moment's respite is to possess it no longer; it is rather to be possessed by it, as is the

case with blind and irresistible instinct. A hen, for instance, is so possessed by its maternal instinct that it is blind, so to speak, up to the point of not being able to realize that a chicken has been added to or substituted for its own. It is the same with the spider at the moment it lays its eggs. It carries the bag with the eggs hanging behind, and never leaves it, however encumbered in crawling about. Would the spider recognize what belonged to it? It is incapable of doing so, Fabre answers. One can replace its own bag by a cork ball; it does not distinguish between the real and the false one. It seizes by chance sometimes its own bag, sometimes the substitute. What it first touches becomes its prize as soon as it is hung on its back. Knowledge or choice, so characteristic, in our opinion, of possession, do not exist here.

Thus one may say that the capacity of the animal to feel itself continuously attached to something sets up constant anxiety, while a man's attachment to a thing is often rewarded by a feeling of detachment equivalent to indifference. It is thus that one can state that the latter, the moment it becomes predominant, brings what may be called the destruction of possessiveness, in consequence of its indifference.

There is no reason to mistake this indifference which comes from the use of possessions with the indifference with regard to possession exhibited by children whose existence is protected and guided primarily by their parents. A child essentially lives under the control of impulses of relatively short duration but high tension. Children do not succeed in attaching themselves to an object and in remaining relatively content, with the result that they have the capacity for irritating and fatiguing their parents and elders after a certain time. One can understand therefore that in similar states of consciousness there is little room for relaxed attention and hence of possessive ties. What children lack in experiencing possession, besides the foresight which comes only from education and experience, is a voluntarily controlled amount of consistency and stability in their interests. They stick to objects when they are under the influence of a process which develops within themselves. Once the latter has come to an end they turn away, they forget, they lose interest too easily in objects which have been used as instruments or implements, in short, as a means to the development of the process in question. To be able to remain indifferent: What a happy age is childhood! Later on, when the adult has experienced the heights and depths of possession, when he has become capable of casting a glance in retrospect on his own existence, how often is he justified in envying this childish indifference. We often prefer fleeting ties, the pleasing ties of high tension but short duration which follow each other as in a kaleidoscope, to the ties which weigh us down even when they are relatively weak. Thus cynics say that marriage is the tomb of love!

IV. The important problem of the origin of the instinct of possession

The problem which now presents itself is to know how to explain certain cases of possession in animals, described by psychologists such as J. M. Baldwin, M. Piéron and Larguier de Bancels; such as the case of the dog which buries the remains of its meal or of the prey which it captured but could not devour on the spot, or that of rodents storing up reserves for the bad season. The above-mentioned psychologists and many others consider these cases as instinctive. To clear up this point, we must return to the question previously raised, namely, what is instinctive and what is not.

As to man, certain people have maintained that he differs from the animals by the

absence of instincts. If that were so it would give the required solution to our problem. For W. James (*Principles of Psychology*, II, 441), on the contrary, the human instincts are far more numerous than those of all other mammals, including the monkey. The apparent absence of instinct in man would be due to the superior ability with which he controls natural tendencies, adapts them to higher ends and to social necessities, sometimes modifying, sometimes even perverting them. The consequence is that the exercise of instinctive functions loses the rigid and fatalistic character shown in the animal; and thus their exercise may be easily confused, in the adult, with voluntary action or with acquired habit. In order to study instinct, man must therefore be observed in circumstances in which inherent influences do not occur. In these conditions, in order to realize better the scope of the instinct of possession, or of acquisition as it is also called, it is preferable to confine oneself to spheres in which this instinct is not unduly complicated, that is to say, in the spheres of animal and child psychology.

Since the child, in contradistinction to the bee, for instance, is not born fully developed, and since the child, according to what has been observed, brings into the world only a small number of instincts among which the instinct of possession does not appear, let us ask this interesting question: "Can one observe the birth of the instinct of possession?" F. le Dantec, as a scientist, confirms this in every respect; and Abel Hermant, as a man of letters, demonstrates it also. "We have previously witnessed", says le Dantec, "the birth of the instinct of possession; it originates naturally in habit and custom and is maintained by the individual capacity for destruction. The cave-man was possessor of his booty when he was strong enough to defend it against his neighbours' greed. The possessive sentiment already very strong in primitive man (it is the same in all animals because they are living and subject to the law of habit) becomes still stronger when there is any question of possession obtained through work" (*L'Égoisme Base de Toute Société*, pp. 231–2).

Abel Hermant tells us in his novel, *La Marionette*, how little Bébert, aged seven, moved by an irresistible instinct of possession, and taking advantage of a temporary distraction among his companions, seized a glass marble. "Suddenly Bébert felt something like a slight shock, and then a keen, even a pleasant, surprise....His constantly wandering glance, seeming to see nothing, previously blurred and giving indistinct outlines to the clearest objects, now became extraordinarily precise and sharp. A few steps away from him three children were playing marbles, and for perhaps two hours he had paid no attention to them. One of these marbles, a glass one, aroused his desires. It was also the first time that strictly speaking he had experienced desire. Often he had made a gesture, but only a gesture, of desire; like all children he furtively seized everything within reach of his hands and kept it. This time it was really the instinct of possession awakening in him, and he noticed at the same moment that it was identical with theft.

"The two essential methods of stealing or, otherwise expressed, of becoming a possessor by force or cunning, were instantaneously revealed to him. At the same time, he learnt fear, but also boldness. By reaction, he felt some touch of prudence; he tried to fight against his hazardous desire, but in vain. He was drawn to this difficult step by the instinct of dissimulation, which awoke in him no less suddenly.

"He began to make movements, still less well co-ordinated and more awkward than usual....He made great efforts...he observed the legitimate owners of the marble out of the corner of his eye. When he was quite sure that no one was noticing him, he crawled

with surprising agility towards the object of his desires and came back grasping it in his closed fist, taking up, at exactly the same place from which he had set out, a position so natural that the most malignant observer would never have thought of suspecting him.

"...When he no longer doubted his victory, he felt an urgent desire to proclaim it, like a cock crowing after mating. Prudence counselled silence, but he wanted at least for his personal satisfaction to throw triumphant glances in all directions."

The tale which we have just quoted is interesting, for it shows how the impulse which we call that of possession arises. It appears to be sleeping while it matures within the individual, ready to reveal itself suddenly, sometimes precociously, at a suitable opportunity. Thus it resembles the sexual instinct, with, however, this difference, that the latter is revealed much later.

V. The problem of the instinct of possession in the child

We are going now to oppose to the usual conception of the birth of the instinct of possession a series of observations made on twin sisters by Dr Maublanc and published in the *Journal de Psychologie* (1938). The observer wanted to know at what moment, in which form, and under what influence, the individual instinct of possession appeared in the child, so as to be able to establish whether or not the instinct in question is innate—not in the sense of an impulse appearing at birth, but of one which on its manifestation seems to be fully developed and instantly complete, that is to say, spontaneously constituted as an internal need without any external influence. Let us say forthwith that for Dr Maublanc, in the light of his observations as to the manifestations of the sense of possession in the child, there is nothing innate, because the sense of possession is aroused from without and developed gradually through the experience of the external environment and the example or influence of the parents. Possession then would not be instinctive in the child. Such also is our opinion.

In order to understand better the reasons for this conclusion, it is advisable here to reproduce some of Dr Maublanc's remarks, based on his observations of the twin sisters between the ages of two and three and a half.

The sharing of toys exists between them not only at home. Their mother takes them every day to a garden square where the girls arrive with their buckets, spades, balls and dolls; but after a very little while they exchange toys without any obvious intention. The other children do the same. So that after a certain time each child is using someone else's bucket and spade. But it is just at this moment and in these very circumstances of which I have spoken, while playing together in a public garden, that the feeling of possession appears or rather arises in its entirety and is imposed on the children by adults. The mother carefully inspects the objects that the child brings back. "Go and look for *your* ball, bring back *your* bucket which the little boy has stolen from you." In a word, if children come to consider certain objects as their own, it is not through a natural disposition but through the effect of education, which reflects the economic system of the society in which they grow up. It is because toys are the individual property of the parents that they become the individual property of the children. This development necessitates the use of language; it begins at the moment when the contact between parents and children is at its closest. A child thinks himself legitimate owner of his coat, his shoes or his exercise book because he will have to account for them to his parents or teachers if he dirties or loses them.

Between 32 months and $3\frac{1}{2}$ years the twin sisters decided upon toys to be shared in common, like the box of bricks, the teddy-bear, etc., and upon a series of personal toys that each of them defended keenly against the designs of her sister. There are therefore two categories of playthings, those which constitute personal property, to be taken to the garden or to be kept at home, and those which are common property.

If one is willing to call this example one of innate instinct, then into what psychological category does it fall?

If one examines the story which Abel Hermant tells of the state of mind through which Bébert passed, in the previous example, one is struck by the fact that this story agrees with the theory of Pierre Janet (*De l'Angoisse à l'Extase*), of the degree of psychological tension relating to the activation of this or that tendency. According to Janet, the same tendency under different influences can become active in different ways and to different degrees. The forms that the different tendencies take in their more or less complete realization are variable, but there are, however, certain common characteristics which allow several degrees of activation of the tendencies to be ranged one above another. A first degree of activation is the state of latency. This is the state in which are the tendencies which exist in the individual in the form of dispositions to certain acts, but which do not actually determine the appearance of these acts to a perceptible degree. It must not be thought, however, Janet says, that a latent tendency is identical with a non-existent tendency or one which has completely disappeared; the stage of establishment is above all a preparation for action. In certain cases the tendency in realizing itself stops at the stage of establishment and takes the form of inner thought, especially after the development of language. Pursuing his analyses of the degrees of activation of tendencies, Janet distinguishes still other stages, namely, that of effort and that of triumph.

How does it happen, asks Janet, that the tendencies evoked reach one or another of these different degrees of activity?

It is not possible to explain these facts simply by external influence, for a very weak influence can, in certain cases, be the starting point of a violent and completed action. The tendency itself can present, according to circumstances, a greater or less disposition to development and to realization. Thus when a tendency has been strongly activated, says Janet, it is less ready to function again. An animal which has just fed does not start eating again as soon as more food is offered. In principle, an isolated tendency has a high degree of activation, as was the case of Bébert in his experience with the marble.

If the marble, instead of being made of glass had been made of sugar, it is probable that Bébert would have eaten it and that there would no longer have been any question of possession. Now in order to find out whether Bébert did establish a possessive tie, it is not sufficient to insist on the fact that he seized the marble while he was dominated by a strong psychic tension. The possessive tie is not only a simple acquisition, a gesture with no future consequences. It is rather a unification.

Now we know nothing about the subsequent history of the marble, if it had a subsequent history. We have seen that Bébert was mature enough to desire the marble greatly and to seize it, but was he also mature enough to keep it? He was capable of an act with a high degree of activity; but was he at the same time capable of keeping up a sustained interest, however slight, in the marble in question, after the force of the tension had relaxed, in order to maintain the state of consciousness demonstrated in the seizure of

the marble—that is to say, a certain continuity that we have called elsewhere relaxed attention? In order to say that the tendency which Bébert had experienced was called forth by the possessive attraction, it must be assumed that the state of latency from which sprang the final state of triumph contained a desire reinforced by an idea; that is to say, that it was not simply a question of seizing the marble, but also of keeping it. In other words, it must be assumed that Bébert was capable of an attention which Mill has defined as "a permanent possibility of sensation".

VI. The so-called instinct of possession in man and animals

But one may say that this preoccupation with the future could equally be revealed in an innate tendency in the form of instinct instead of an idea. Is it essential, one might ask, that this preoccupation should be acquired? Baldwin, for instance, maintains that there is reason for admitting that possession is "a native tendency" both in man and animals. "Psychologically", he says, "the acquisition impulse (or instinct), as it has been called, seems to be rooted very deeply and demands recognition. The existence of this instinct is a refutation of the opinion which considers the rights of property as something conventional and artificial. Even with animals one finds the recognition of *meum* and *tuum* and that not only with regard to other individuals like the young of the family, but equally with regard to things. The bird claims the nest and even the whole tree as its own, and the dog defends its kennel with its life. In certain cases, as with the squirrel which lays up provisions of food, the instinct extends to future needs. Certain birds like magpies even appropriate useless objects and consider them as their own. With children this impulse develops very early. It must be considered as an innate tendency though, in spite of the fact that doubtless it owes much of its strength, as well as the direction which its development takes, to example and social education. Its utility is so great from the genetic point of view, for it extends into all corners of the personal life at its first stage, that its survival and evolution seem to be simply a great sociological fact" (*Dictionary*).

It is probably the lack of precision in what underlies the idea of possession which explains the ease with which Baldwin qualifies as innate the different examples of possession which have been described. But let us return to a psychologist like Larguier de Bancels, who takes care to express himself more precisely and exhaustively on the instinct of possession. In his opinion there is in the individual a kind of irresistible tendency to amass objects. It is in this tendency that the depth of man's longing for possession is revealed. "He seeks especially to do everything that he can to obtain and to keep" (*Traité des Facultés de l'Âme*, II, 527). Thus it may be held that the miser is the man in whom the desire to possess has reached a frenzy. Although Larguier de Bancels recognized that riches may be considered as a means, especially as a means to power, "it remains nevertheless very probable at the same time", he says, "that the desire for possession corresponds in innumerable cases to an original tendency, and notably that the miser amasses merely for the sake of amassing and not in order to gain power". If this were so, the eccentricity of this tendency would express itself above all in a kind of greed without any regard for expediency. One might doubtless say that the tendency to amass, whether it is original or derived, results from an imperfect capacity for foresight in the individual. A man who cannot foresee with accuracy what he will need, gives

himself a margin of safety by accumulating and amassing. The imperfect sense of foresight would then afford an explanation of the tendency to amass in so far as it is excessive and irrational. One can imagine other reasons for this tendency. Balzac maintains that in provincial life one takes pride in one's wealth and not in one's expenditure. Ambition fades and turns into avarice for the lack of the wherewithal to feed it, and doubtless from other reasons. We think, however, that it is not reasonable to conceive of the desire for possession from a single, negative aspect. In the feeling of 'mine', for instance, the desire to preserve is not equivalent to the fear of loss. Thus the mother, in the celebrated judgement of Solomon, preferred to give her child to another woman who claimed it, though she was not its mother, rather than see it die.

If the instinct of appropriation is to serve man and not to render him disservice, if it is even to help him onwards ("for man", as Larguier de Bancels says, "is not content to defend his existence, he forces himself to develop it, to increase and enrich it"), why therefore conceive of this instinct as a sort of monopolizing? Why not admit that there is in appropriation a faculty of discernment combined with an appreciation of opportunity? In my opinion there is no necessary genetic relation between amassing and preserving. I am inclined to see in the tendency to amass, so keen in the miser, an error which consists in confusing the means and the end. The individual, in order to develop himself, makes use of means—external objects which facilitate his development—to which he devotes his interest. In so doing, he makes mistakes which he corrects. In short, he feels his way about among his experiences before he attaches himself to the objects on question. One calls this 'feeling one's way about' explorative activity. To accumulate, to amass, to monopolize, is only of importance to the individual's existence if the instinct of appropriation, in so far as it does exist, appears in a positive form. Is it besides indispensable to define the desire to possess beforehand as an instinct? Is it not enough to assign the responsibility for this function to the intelligence, the action of which may eventually become automatic? Thus according to McDougall (*Outline of Psychology*, 1923), instinct and intelligence never represent two divergent directions of evolution, nor even two stages of it. They are merely two different aspects that we distinguish by an effort of abstraction.

When the spider spins its web or the bird builds its nest, the psychologist explains these acts in terms of simple physiological functions. It is said in this case that the animal lives under the influence of impulses or of events, that it lives in a disjointed manner, one thing after another. It is subject to events, instead of directing them. According to C. Poyer (*L'Activité Mental, Travail Intellectuel et Fatigue*), activity in the animal is directly dominated by natural needs, instincts of preservation, nutrition and reproduction. When these needs have been satisfied, activity ceases for a while, to be resumed somewhat later. One sees a series of impulses following each other calling up a series of acts closely connected with the situation of the moment. In *homo infans*, the primitive man, one finds again this domination of irresistible impulse uncontrolled by any rule and satisfied only after the accomplishment of some rather simple act. From his childhood, man is brought up in a social environment which affects him more and more. The civilized adult obeys rules and regulations more often than his instincts and his true psychological tendencies, and the ideal of many is even the subordination of instinct to social regulation.

Is this also the case with the ant, which gathers provisions and procures means of

subsistence for the future in order not to suffer want, this being exactly the function of possessions? Ch. Blondel thinks that in order to explain these cases, account should be taken of the occurrence of a state of existence other than the material one in the animal world. Do not the social species especially, he asks, introduce a reality of another kind between the animal and its material environment, the beehive for the bees, the ant-hill for the ants? Although on that point we are still confined to conjecture, for certain insects a truly collective state of existence does occur. It must superimpose itself exactly upon their material environment, for the species seems once and for all to have adapted its collective form of existence to its natural needs in such a way that the social life of the bee and the ant, unlike that of man, is in harmony with their physiology. The social life of the insect must be identified with the existence of the species. Thus the constitution and the isolation of a new milieu intermediate between the material environment and its organic activity are still premature.

Besides, it is not always easy to identify in the animal the instinct in question in any particular case. We do not refer to the law of fusion of tendencies, but to the difficulty of interpretation. A way of escape from this difficulty is to assume as is usual the existence simply of an instinct of preservation. This instinct with its multiform aspects offers an easy way of covering all cases without defining more precisely the kind of instinct in question. Let us take an example. I enter my hen roost, in which are a cock and ten hens. I seize a hen and try to get away. But, lo and behold, the brave cock jumps at my throat. I let the hen go and peace is re-established. What exactly is the instinct which reacted in the cock at the sight of my indelicate act? It is the instinct of possession, someone will say. No, it is the sex instinct, another will say, for it was a question of a favourite hen. Not at all, interposes a third, it is surely the instinct of defence of the species which has been aroused, for there remained nine hens to satisfy the sex instinct of the cock—always supposing that the cock is capable of doing the sum. A casual observer would conclude that it is all these instincts at once, for it is simply a case of the one instinct of preservation which includes them all.

For Baldwin, instinct is "a hereditary reaction of a sensorimotor type, clearly dynamic in purpose, and common to a whole group of individuals". Baldwin sees in the fact that the dog attaches itself to its kennel and the bird to its nest the presence of an acquisitive instinct. But can one not see in it a simple habit, an acquired tendency which has been added to an instinct according to the law of the individualization of instincts, formulated by James (*Principles of Psychology*, II, 394)? One will always see, observes the latter, how the limpet returns and sticks to the same spot on the same rock, the lobster regains its favourite hole at the bottom of the sea, the rabbit deposits its excrement in the same corner of its warren, the bird rebuilds its nest on the same branch.

But the desire for possession, as I have remarked above, does not show itself in one form only. It is complex and multiform. One insists on the monopolizing tendency in the instinct of possession, but one can also insist on that of absolute possession which allows of no restriction, on that of perpetuity, and especially on that of exclusiveness.

Colette, the well-known writer, claimed to find the presence of this sentiment of exclusiveness in the attachment which her dog showed her. It was apparently jealous even of a child's tender embrace made in its presence. Submissive, self-effacing, powerless to protest, it shut its eyes to overcome its pain at this example of the fickleness of human friendship. The case reported by Colette is not an isolated one; but supposing that

observation and interpretation were correct, it would be rash to conclude that dogs were animated by a feeling of possession. In any case, the dog was not frustrated in its feeling of possession but in its feeling of belonging, which is not the same thing. But, it might be said, if one is capable of showing passive feeling, is one not equally capable of an active one? Jealousy which reveals itself in the desire for exclusiveness is the expression, one might say, of the dog's fear of losing the affection of its mistress. This is in the order of things. What would not be so is the opposite case. In the case observed there is no possessive tie in the direction 'dog—mistress', but there is one in the opposite direction; the dog was incapable of mastering its protector. Because of its lack of strength, it showed faithful submission.

The right of possession in the accepted meaning of the word is an exclusive right, perpetual and absolute. Bolshevism, in instituting collective ownership instead of individual possession by declaring that objects belong to everybody in general and therefore to no one in particular, has struck at the heart of possession; for community is the worst enemy of exclusiveness. To possess in common reduces itself to the negation of individual and therefore of exclusive possession. Bolshevism has been even more radical. It has advocated the destruction, the annihilation even, of the objects which constitute possessions. It is not so much a question of attaining equality of riches, but rather of equality of want. From the point of view of exterminating the feeling of possession, this last policy seems more fruitful in its results. But nothing can be perfect in this world. There are always things indispensable to individual life which cannot become objects of common property. I do not mean the tooth brush, since that might perhaps be open to discussion; but we might agree more easily upon the case of the shirt, for instance. As long as men do not denude themselves of everything, as do the dervishes, it appears that they will always pocket things and endanger the absolute collective ideal. The germ of exclusiveness will never be exterminated, and the sentiment of individual possession will persist, thanks to its influence.

In short, one is a possessor or one is not, but from the moment when one has become a possessor, the states of consciousness to which we have given the name of relaxed attention establish themselves in relation to the objects possessed. They live in us in a manner enduring and well-organized but not unintermittent. It is this which distinguishes them from desires, including the desire to possess, for unlike the latter their function is not to serve as ends but as means.

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SYMPOSIUM ON THE PSYCHOLOGY OF MUSIC AND PAINTING

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I. *Introduction, by R. W. Pickford* (pp. 40–42). II. *Musical inspiration, by W. Gillies Whittaker* (pp. 42–47). III. *Pictorial inspiration, by William O. Hutchison* (pp. 47–53). IV. *Conclusion, by R. W. Pickford* (pp. 53–56). *Appendix, by R. W. Pickford* (pp. 56–57).

I. Introduction. By R. W. Pickford

In this symposium we have tried to put before the psychological reader some of the aspects of pictorial and musical art as they present themselves to the artist himself. Prof. Whittaker has dealt with the musical aspects and Mr Hutchison with those of pictorial art. A questionnaire, printed in the Appendix, was the basis of their statements. It is easy to read accounts by other people of the way artists are supposed to work, and psychological interpretations are freely to be found. In the present symposium Prof. Whittaker and Mr Hutchison have given accounts at first hand, and their own psychological interpretations. They have not been prejudiced by previous theorizing or by inclination towards or away from any special psychological doctrine. In the conclusions I have avoided critical or prejudiced discussion of what they have said, and have done my best to let them speak for themselves. It would be easy to make lengthy analytic comments on their statements, but in the present symposium we wish interest to be concentrated upon the actual experiences and interpretations which they have offered themselves. This symposium might be the beginning of a more extended study of the same kind of material.

In their statements, Prof. Whittaker and Mr Hutchison will be found to have followed the main outline of the questionnaire. Its four problems are: (1) Objective conditions; (2) Subjective conditions; (3) Mode of inspiration; (4) Technique and convention. Since the authors do not follow the questions in detail, and, indeed, were not intended to do so, the questionnaire has been put in the Appendix, but the numbered sections of their contributions to the symposium will be found to correspond to the four headings given. This is a free use of the questionnaire technique, when its object is to stimulate responses in certain directions rather than to obtain exact answers.

The formulated questions enabled the statements on music and painting to be co-ordinated without suggesting that the psychologist would expect any particular kind of response. That this is true will be apparent to anyone who cares to study question and answer in detail. He will be surprised at the nature of the answers given in many cases. The point of view taken up by artist and musician is therefore refreshing to the psychologist, who is used to conventionalized formulations. For example, Prof. Whittaker answers the extremely commonplace psychological question whether invention occurs in a concrete or abstract manner by saying that it occurs in a musical way. Mr Hutchison, without any previous collaboration about the way to answer such questions, makes almost the same observation about pictorial art. Thus, by a bold stroke, each wipes clean the psychological slate.

It is not possible, of course, to write in general, as if for the universal musician or artist, because such a person does not exist. Clearly, from the start, every creative artist works in his own peculiar way, and psychology could not make it its business to draw generalizations applicable to no artist in particular or to a laboratory dummy. The psychologist must always think of the individual who actually feels and invents in his own manner. Moreover, while personality cannot be expressed purely in terms of the peculiarity of the individual, because in conventional individuals conventional personalities are found, yet in a matter like creative art, outstanding gifts and distinctive qualities are emphasized to the full, and so the psychology of creative art brings forward the study of individual personalities in a prominent way.

Uniqueness of individual attitude has been stressed considerably in this symposium, but neither Prof. Whittaker nor Mr Hutchison is able to give complete psychological accounts of the manner in which his ideas come into being. They are not willing to pretend that their accounts are in any sense complete. Only snatches of the process of invention have been caught and expressed in words. Even if these authors were convinced that their accounts could be made complete, they would be unwilling to publish the details. This reluctance to submit to analysis is partly due to an attitude which is more than a conviction, which may be expressed by saying that the artist wishes to make things for other people rather than to ponder over his own thoughts. He will express himself with delight in his paintings and his music, but not by trying to say how he makes them. Mr Hutchison says that his art is so much a part of his own being and existence that he finds it difficult to disentangle what belongs to his life and what belongs to his art. Prof. Whittaker approves of this way of expressing the matter.

Another attitude which draws these artists back from the exhibition of their inner experiences is the conviction that no amount of explanation in words of the meaning of an artistic idea can be a substitute for the necessary understanding itself. It is natural, for instance, for the painter to take up a brush or pencil and start painting or drawing, and while Mr Hutchison might be able to demonstrate to students some of the things he is aiming at, yet he tells us that he knows by bitter experience that these demonstrations, even if backed by the spoken word, do not convey even to the most sympathetic student the underlying urge which sets him going. To attempt to disentangle his art from his life makes the painter feel a little as if he were undressing in public.

It is possible that a poet or other literary artist would say the same, because prosaic analysis is no more able to replace verbal than pictorial or musical art, though it will be agreed that discussion of artistic meanings and ideas illuminates them to a certain extent.

While this symposium aims at an expression of the artists' individual personalities to some extent, it is nevertheless greatly limited, and no apology is offered for the limitations. In another respect it attempts to throw some light on the artists' outlook and position in relation to other people. The individual's experiences are presented in view of his membership of a society interested in painting and music. In many places it has been possible to make generalizations which are to be viewed as the results of knowledge and experience rather than as abstracted psychological laws or principles. This symposium must not be looked upon as an attempt to dissect the creative artist to the very last stages of scientific examination, for, if viewed in that way, it will certainly be seen to have failed. It is an attempt to represent to the psychologist something of the

musician's and painter's own impressions of his work. If taken in this way, it may appear as an illuminating picture, subject, of course, to limitations which are freely acknowledged.

II. Musical inspiration. By W. Gillies Whittaker

(1) In dealing first with the objective conditions of musical inspiration, it is important to say that music has in the last hundred or more years been greatly influenced by folk art. This is a natural source of musical ideas. The music of the concert hall and theatre can be cultivated until the genius of their material is lost in the production of themes and settings which are over-refined in conception and treatment. Then a return to the sources of spontaneous folk music renews the vitality of inspiration. It is because of this need for renewed vitality after the period of great classical composers, that so many modern musicians have turned to folk music for ideas. Folk music, being the great reservoir of human musical interest, is also a mainspring of cultured invention.

It must not be thought, however, that mere tunes written out would be sufficient results to be gained from the inspiration of folk music, though much valuable work has been done in the collection of folk tunes. The great interest of the original composer lies in giving the tunes their appropriate musical settings. Any student of music can add harmonies to a folk song, but it takes a good composer to furnish an inspired setting from his own artistic imagination. The particular tunes which happen to excite interest at a given time may need to dwell in one's mind for days and thus become absorbed into the creative side of the personality. After a long period of this preoccupation with them in a half-conscious way, they may re-emerge with their true musical settings. Thus the setting is not a mere harmonization or accompaniment, not just a question of fitting tunes together contrapuntally, but is a vital product unified with the melodies by creative imagination.

In the invention of music the presence of possible performers is of great interest and importance. It is not so much a matter of considering what one could write to suit such and such a flautist or violinist, but the imagination is stimulated by observing the powers of player and instrument, and this leads on to the use of those powers in a new and creative way. The mind responds to the impression gained from hearing a given performer and instrument, and tends to find new ideas, new ways in which they might be even better employed. Thus the composer, not the performer, is the real master of music although in popular imagination the singer or player of virtuosity is often granted an unjustified credit. The composer thinks of musical ideas which are often considered to be impossible by players whose contact with him has actually set them going in his mind. Later, it is found that new fields of expression for that instrument have been legitimately explored. While the invention of music may be inspired by accepted ideas, it is often occupied in practice with the expansion of these ideas into new and unforeseen shapes and forms.

In comparison with the presence of performers, the audience is of lesser importance. Music is perhaps greatly influenced by the idea of interesting many people who may listen to it, of course, but it is possible for remarkable compositions to be produced when there is little hope of their being performed before big audiences. Public enthusiasm may come later; often it comes long after a composer's death, when manuscripts are discovered which may never have been performed. But the composer is lucky who can

find sympathetic players to try over his works and fill his mind with ideas of the way in which his funds of creative imagination might be employed.

Buildings and scenery have varied effects on inspiration, and it is not possible to give any general principle covering the problem of the inspiring or uninspiring effect of outside objects. Their effect is unexpected and cannot be foreseen. It is possible, for example, to recall a long visit to Italy when it might have been expected that the limitless art treasures of the past would have stimulated invention, but manuscript paper was uselessly carried from town to town and remained untouched. It is not easy to explain an experience such as this. Perhaps it was due to the excessive heat in summer Italy, but similar failure of musical invention can be recalled when visiting colder climates. I have found long walks on the Northumberland moors very inspiring, especially when working upon Northumbrian folk tunes. Sea voyages are generally exciting to the imagination, and one could almost rely on a sea trip to stimulate composition of music. Modern sea voyages, however, are utterly spoiled from this point of view by the endless nuisance of the gramophone, or by a loud speaker always carefully placed so that everybody can hear it clearly.

The following combination of circumstances may perhaps be found interesting. It was on a voyage to Australia. During the voyage I had been reading Herodotus and the long account of the defeat of the Persians by the Greeks, which finishes the book, had made a deep impression on me. We had crossed the equator; owing to the monsoon the sky had been clouded for some days. On the evening of the day on which I had ended the book I went on deck and saw the most marvellous sight of my life. The sky was ablaze with stars, the Southern Cross the most luminous of all. They were so brilliant that one felt that one could almost touch them. Below, the sea was equally ablaze, with phosphorescent lights. As far as the eye could reach the waves seemed to be made of molten silver. I gazed spellbound for hours and went to bed almost drunk with the sheer beauty of it all, only half-conscious of my surroundings. Next morning I came across William Habington's poem 'The Coelestiall Sphaere':

When I survey the bright
Coelestiall sphaere:
So rich with jewels hung, that night
Doth like an Æthiop bride appeare,

My soule her wings doth spread
And heaven-ward flies,
Th'Almighty's Mysteries to read
In the large volume of the skies.

It tells the Conqueror,
That farre-stretcht powre
Which his proud dangers traffique for
Is but the triumph of an houre.

That from the farthest North,
Some Nation may
Yet undiscovered issue forth
And oer his new got conquest sway.

Thus those Coelestiall fires,
Though seeming mute,
The fallacie of our desires
And all the pride of life confute.

For they have watcht since first
The world had birth:
And found sinne in it selfe accurst,
And nothing permanent on earth.

After reading it three or four times I went to my cabin, got some music paper, and returned to my deck chair away from everybody. I started writing immediately, and, in a few days, set the poem for chorus and orchestra, the most elaborate and harmonically

involved work I had ever written. Composition was extraordinarily rapid, ideas came spontaneously, and when I got on shore and tried it over on a pianoforte scarcely a note needed to be altered.

(2) Some of the objective experiences which may stimulate invention have been indicated, but it is not possible to explain in words what musical composition is trying to express. Music starts where words end, and not only is it impossible to explain the meaning of music in words, but I am not willing to try to do what is certain to meet with failure. In composing in Sweden an Adagio Introduction for a Sextet for violin, flute, bass oboe, horn, bass clarinet and piano, for instance, it seemed that something of the feeling of vast spaces was in the musical idea. This was, however, not solely in the theme itself, and could not be illustrated by playing that tune on a piano, or even by trying to render the whole composition on this instrument. The feeling of vast spaces was in the musical colouring and texture of the piece as a whole, so it is useless to try to illustrate this without the actual instruments whose combined qualities were appropriate to the idea. In any case, the words 'feeling of vast spaces' do no more than crudely indicate the nature of what was being expressed. The musical treatment was the only possible way of saying what was felt.

More objectively speaking, the urge to compose, as you may have gathered from my earlier remarks, often takes the form of a desire to provide something for other people to enjoy and value for themselves. It is a striving to give out what will add to their lives, something of new experience and of creative interest for them. What that something may be we cannot say, except by doing it, but the whole value of the work of composition would be lost and would indeed become pointless apart from the urge to do something for other people. In this the composer often may be disappointed, because of his incapacity to fulfil his own wishes, and also because of other peoples' inhibitions, which make them incapable of valuing what he does even when it is successful, but the urge to create persists in this objectified form nevertheless.

The composer, in my experience, is not any particular type of man. Composers are as varied as plumbers or bricklayers, but, whereas the plumber remains unnoticed, and can live his own life much as he wishes apart from his work with lead, tubing and solder, the composer, especially if successful, is publicly examined from all angles, criticized and studied. One of the reasons why there is much reluctance to give an account of musical inspiration, is a profound doubt about the true relevance of personal matters to the real work of musicianship. The public, in their zeal for details about great artists and musicians, throw out their own fantasies upon those unfortunate personages, so that, for instance, it becomes almost impossible to find true information for a reliable biography of so recent a musician even as Wagner. Perhaps one quality of the great musician may be noted here, and that is his wish to vanish for himself, and to be understood through his musical work instead. Thus we find that the real artist is seldom to be seen. He is working too hard. Charlatans, inferiors and nincompoops in the musical world may be found vaunting themselves in the public eye, but the real musician is invisible to the crowd, at his desk surrounded with manuscript paper, or at his rehearsal surrounded by players and singers.

(3) The question whether musical inspiration presents itself in concrete or in abstract terms is not understandable to me. Since music is made up of sounds and consists in the artistic use of sounds in procession and combination, how would it be possible for

musical ideas to be conceived in abstract terms? They can only appear in the form of sounds. There is no possibility of any musical inspiration being an abstract experience. It is either auditory or nothing at all. The signs used in notation of music may be regarded as musical abstractions, but they are merely ways of indicating the sounds to be employed. In some early manuscripts it is not possible to read the notes directly, as we would in modern written or printed music. We have to interpret the signs first into modern notation, but the result is an experience of sounds all the same. It is not possible for any musical inspiration to occur in other terms than of music. Some composers may be much affected by the appearance of music on paper. For example, a modern composer might like to write a particular passage in terms of breves and semibreves because of their association with early Church music, if his piece seemed to have something of the spirit of plainsong. This notation would be more easily read by players of to-day if translated into the minims and crotchets conventional in modern music, but the appearance of the composition in breves and semibreves might be suggestive to the composer. This illustration was suggested by passages in the last movement of Magnard's quintet for wind instruments and piano, but the essential principle holds true of other works as well. Such a love for the qualities of the notation itself is perhaps not a fundamental aspect of musical inspiration, but is of some interest nevertheless, and is also implied by the indifference or dislike which many instrumentalists feel for the sol-fa system, saying that it is not music at all.

As for the problem of delay and criticism, it seems that musical composition is necessarily subject to great delays. Music is not like pictorial art, in which one can see the whole of a drawing as if in one view. It is continuous and expands or unfolds itself in the imagination. When we start out on the beginning of a composition we must persist forward until we have developed the music to the end. If an analogy with painting is needed, music is like the Chinese pictures which are rolled up and are not intended to be seen all at once. They must be viewed section by section as they are unwound at one end and wound up at the other, although, necessarily, there must be unity in the whole. In composition of music it is not always possible to start with a sketched outline of the whole and then fill in the details as in an easel painting, but a vast amount of work is necessary to compose right through to the end. Months or years may be spent in this way on a big score, and by the time it is finished, the product is no surprise. Some composers, illustrating a partly finished composition to a friend, might be able to show sections complete and for other parts simply say, "It will be something like this", sketching it out on the piano, but the completed product is not finally thought of until it is finished. Other composers may be able to say, for example, "I have just finished my third symphony", meaning that all now necessary is to write down the notes already completely conceived in imagination. Nevertheless, much time will have been spent in working out the structure, detail by detail, in the mind.

In the warmth of inspiration one often has the impression that the products are of outstanding merit and one is delighted with them, but on careful consideration after the enthusiasm has waned, they may appear valueless and need to be put on the fire at once. Sometimes it is easy to overestimate the quality of a work even when it is undoubtedly a good composition. Ideas in imagination may seem far more brilliant than the score when complete. There is always the possibility of being quite unable to render into actual notation the vividness of an inspiring idea. Whether this is due to the

limitations of the technique of writing and expression, to the composer's own technical limitations, or to the essential futility of the apparently brilliant ideas, it is not easy to say in every example.

It is not possible for me to sit down and will the composition of a piece at any chosen time. Some composers are able to have habitually fixed times for work, to start, say, at 10.0 a.m. and continue exactly where they left off the previous day at 5.0 p.m., and it works well with them. For me it is usually necessary to be ready to seize the inspiration when it comes and to work fairly continuously until the composition is more or less complete. It is not possible to know beforehand when inspiration will come.

The piano is necessary in a sense, and in a sense it is not. Often it is found on playing over a work that certain parts will have to be altered, where one has overstepped the limits of adventure too far, but to a large extent works remain unaltered after testing on the pianoforte. Of course this is not true of all composers. Some will be found whose inspiration depends greatly on opportunity to go to a piano and play over many works by other composers to set their ideas flowing freely, and also to improvise themselves as well.

(4) Music which is not composed in such a way as to fulfil technical requirements is useless and would never be played. No composer who wishes his works to be performed would write passages impracticable for the instruments he has chosen to employ. It is a necessary condition of composition that we should accept such limitations. At the same time, as pointed out before, the composer often sees how instruments might be used in new and unexpected ways, which may be resisted or regarded as impossible at first, but which later become parts of the accepted technique for that instrument. It is in this way, for instance, as well as in elaborations of instrumental technique by virtuosi, that the potentialities of the violin have been unfolded in the last three hundred years. It is the same with the technical limitations of counterpoint, harmony, formal qualities and other aspects of composition. Technical requirements are fulfilled by the composer, who, nevertheless, by adaptation and adjustment peculiar to his own feeling and inspiration, introduces a flow of new invention which makes his work fresh and expressive of his own manner of outlook.

Perfect, neat and legible scores represent the end-products of much writing, crossing out and readjustment. Some composers are able to do this purely in their minds, and do not set a note to paper until satisfied with the final shape of the product, but most are able to compose only in writing the music down. There is an intermediate and frequent way of working, in which the written material is subject to little alteration, but is the product of much experimentation in mind as the work goes on, though the complete composition may never be present to the mind in its final form until it is also fully written out. Some further adjustments may be needed when the piano or other instruments can be used to test the work. The popular idea of the composer as a person who is swept on by torrents of invention which come in perfect form, and who needs only to take a pen and paper and write it all down, is altogether false, except perhaps in the case of Schubert. Musical composition is very hard and laborious work, and perhaps one most vital quality of the greatest composers, such as Bach, Beethoven or Wagner, has been organizing ability. Many people may have effective and even brilliant ideas for composition, but never carry them to completion because they lack the capacity to persist at the tedious labour of detailed work. In the case of a big orchestral score,

as I have said, this labour is very great, and may take years of persistent work, during which many disappointments with the result and difficulties in its formulation must be overcome. From long experience in teaching music, it seems that those pupils who have exceptional powers for sustained effort are most likely to be successful, rather than those who have most brilliance and facility.

In a sense, composition cannot be taught, though the technique can be acquired by instruction. But a composer must *learn* how to compose, whether he does it unaided or by the help of a master. Most people require external help to gain a knowledge of a foreign language. A few have the peculiar mental faculties which enable them to learn it for themselves. There *are* rare instances of composers who have mastered their art by their own studies and self-discipline and self-criticism. Elgar and William Walton are modern instances. But the reverse is more common. It must be remembered that a musician is immersed in music for many hours a day. If he has inclination towards composition, he is continually learning from the music he encounters, noting, whether consciously or unconsciously, what factors go towards satisfactory works, the cause of weaknesses in others, points of style, balance, key system, colour and what not. Unless a man go through the mill, unless he work incessantly, acquiring experience, skill, malleability of thought, experimenting, testing, viewing his work from all sides, he can never write satisfactorily. Many amateurs seem to think that all they need to do is to jot down a few ideas, most of which are merely reproductions of what they already know. No one expects to be able to pick up a brush and paint a masterly picture without first training his brain and his hand, or to be able to pick up a fiddle and play a tune straight away. We remember that Beethoven was taking counterpoint lessons at the age of 22, and that Schubert, just before his death, was planning to have more instruction in the same subject. Technique of composition is as much a science as an art, and years of hard and soul-searching labour and mental discipline are an absolute essential.

III. Pictorial inspiration. By William O. Hutchison

All my important knowledge is contained in that part of my brain or spirit which controls my hands and eyes, and which finds much less satisfactory expression in words than in painting. It may then be unwise to try to understand in words what I already understand in a medium much more suited to my make-up. That does not mean to imply that the painter is to be regarded as unimportant, or that he is a simple child of nature, over whom "impressions are driven, like the alternations of an ever-changing wind over an Aeolian lyre". Very much the opposite, the painter, granted he has the initial gifts, learns and studies through his eyes and his hands, until he reaches his full stature. He absorbs in this way as much as the writer, the philosopher or the scientist can from books and experiments. What he absorbs can only be expressed in his art, but affects his conduct of life, and becomes part of the personality of the painter. How else can we explain the reverence which we feel and which is everywhere conceded to such a man as Rembrandt, except that his was a brain and soul of outstanding eminence, fully developed by the practice of his art, and perhaps if we criticize his daily life we may find that his final philosophy of poverty was not the result of circumstance or even neglect, so much as the deliberate turning away from the pursuit of worldly riches which he had followed early in life, to the finding of untold wealth in poverty and humble

circumstances. In this, as expressed in his later paintings, he comes into line with philosophers and religious thinkers, and, neglected by the fashionable in his day, becomes a corner stone for later generations to build on.

The reproduction of a Rembrandt picture is no more a soporific decoration to be hung on a drawing-room wall than is a van Gogh. Both are used in this way, the former by one generation, the latter by a different generation, but both are full of qualities which rendered their creators outstanding personalities. If, however, we try to express these qualities in words, we are likely to obtain banal literary platitudes and catchwords, explanatory phraseology which if taken literally acts as a snare to would-be followers.

(1) In considering the objective circumstances which prompt and guide the creative artist we have, to begin with, a separation to make. How much is the painter a creative artist and how much a tradesman? Some would tell you that he cannot be both, but I hold that he is both, for even if we take the extreme case of a painter who accepts the philosophy of poverty, he is still a tradesman to the extent of bartering his productions for the humble garret in which he may choose to stay or the simple crust with which he keeps body and soul together. Nor do I know of a painter who has refused to accept a fair price for his work, nor indeed who has refused a price because it was too high. It is perfectly true, nevertheless, that the painter is often generous with his work, and will give it away where he feels that there is genuine appreciation.

The painter, however highly he may rate his creative capacity, and however seriously he takes his art, must regard himself as a tradesman, a worker whose job is to supply a need to other people. If that need is not recognized, if the public are not aware of the lack in their lives, and cannot be awakened to it, then that is just too bad, and the painter has no redress. He must either suffer in silence, create a possibly artificial stimulus for his work, or look for new fields where his gifts can be employed and the public interested. Fortunate is the painter who lives in a time when his own creative work is also a genuine want on the part of the people.

History presents us with many examples of painters who were both creative artists and tradesmen, as well as the contrary picture of painters who were creative artists but not successful tradesmen either because of the time in which they lived or because of some personal idiosyncrasy which prevented the combination. But history does not give us, to my knowledge, instances of creative artists who did not try to sell their work. The successful men have usually been those in the forefront of experiment, men like Michaelangelo, Velasquez, and to-day, Walt Disney. Leonardo presents a slightly different picture. Although he was in the forefront of experiment in his day, and was held in the highest possible esteem, he was not altogether successful as a tradesman, his finest works sometimes perishing in his lifetime, while his financial affairs were usually complicated.

It is therefore not unnatural that Leonardo wished to raise himself and his brother artists above the rank of tradesmen, and would have them classed as philosophers. But in practice he was humble enough, advertising himself in his letters to possible patrons, and hiring himself for his possible usefulness in either peace or war. In doing so he proved himself a great creative artist.

Velasquez, experimenting with light and colour, was locked up in a royal court hidebound by court conventions on every side, yet while satisfying his narrowminded royal patrons to an unusual degree, he extended the science of painting as known in his day, and was regarded by as recent a master as Whistler as the paragon of all painters.

The finest periods of creative painting were in the days when artists were tradesmen and members of guilds, before the attempt to raise them to a higher status under the names of academies and before these present days when individual anarchy leads the creative artist to be a kind of exhibitionist. From this it is clear that I think other people's interest and definite commissions do act as a stimulus to creative work.

It is perhaps not sufficiently realized that up till the seventeenth century anyway, and after that in many cases, nearly all creative work was definitely commissioned. Consider the work for the Church during the Renaissance; even such secular pictures as Botticelli's *Birth of Venus* and *Spring* were commissioned, and the subjects chosen by one of the Medici from poems written by the court poet. Yet to-day this comparatively mundane origin of these pictures is forgotten in the creative achievement of Botticelli who without that stimulus might have done nothing at all. Again, if we think of the decorative or historical pictures and portraits of the French School, and in Britain of Reynolds, Gainsborough, Romney and Raeburn, all depended on commissions.

It may be objected that much of this work was not creative, but purely representational, but in fact all pictorial work that is worth while depends not on its representational power but on relationships of form and colour. What is the difference between a fine Madonna and Child by Mantegna, Bellini or Raphael, and the commercial article that can be seen to-day in certain shops for the furnishing of churches? The difference lies in the creative power of the painter, and without the stimulus of the order he might never have carried out the work at all, for the very good reason that the artist is probably tired of painting Madonnas, or portraits, and is longing to try his hand at some ambitious composition which he cannot quite reduce to concrete form, so he falls back, if he is at the moment unemployed, on apples on a plate or some such other still life, and in spite of the subject and unemployment, will yet produce creative work.

So here again we have a difficult knot to untie, but for myself I have a strong belief that the actual commission of work, and the knowledge that it will be seen and criticized, is a stimulant to the creative artist, particularly in those earlier years when he is not sure of himself and needs just that appreciation to justify his choice of a profession. In later years, no doubt, when he is more sure of his own powers, and accustomed to the praise of his friends and the misunderstanding of those who think differently, he may well take a philosophical attitude and, letting the world go by, produce for his own satisfaction, for research into creative possibilities, and only lastly in the hope of getting a modest return for his work in the way of money. It is in this light that I regard the later work of Rembrandt, which is also his finest work, but his early success and the devotion of his friends and pupils was something that was necessary to his later attitude. In any case, it is probable that he never ceased to sell his work, and his second wife and son were his business agents.

So much for the stimulus of commissions, but what makes the painter want to produce at all? There I must ask the psychologist a question. If he can tell me why a child wants to draw or why a message boy wants to deface a nice white wall with either the scribblings of the written word or the graphic line, then I can tell you why we want to paint. Certainly in most painters, though there are exceptions, the urge goes back to early childhood, and many children, not by any means all likely to be artists, draw just as well as or better than they write to begin with, and prefer it. But the influence of education is thrown almost entirely on the written word, and this soon advances far

beyond the native power to draw. Sometimes, however, the pleasure taken in drawing has been so great and the delight in the visual image so strong that it becomes a ruling passion, and it is mostly from this type that the painter emerges, but he does not emerge as a fully developed genius right away any more than a poet or a prose writer. Both his judgement and his technical powers must be developed. He may produce extremely interesting and precocious work almost amounting to genius. This may by reason of its naïvety and fresh contact with young life exceed in value much pretentious stuff produced by highly trained though uninspired professional artists, but it does not compare and never will compare with the work of recognized masters, who retain the original urge and have added to it what Whistler called "the knowledge of a life-time". This knowledge is not gathered by the reading of theories in words, but by looking and doing, and that means both looking at nature and looking at the work of masters, and doing that is wordless but inspired both by nature and the masters. If this is being done the brain is much too busy for words and an impression of outside inspiration or possession may be given.

Originality is a much-abused word and nowadays can only be used in a comparative sense. The wall paintings in the caves at Altamira or Negro sculptures are probably as near original works of art as we are likely to find, but even there we do not know what may have gone before. So-called originality to-day is either a deliberate attempt to be different which is not likely to succeed in conviction, or a genuine purity or simplicity of mind that sees through convention to the main stream of work that matters not to one generation but to many.

It is thus that the history of art gathers in to the main stream such names as van Gogh and Cézanne, to the exclusion of their much more skilled and pretentious contemporaries whose fame rested on some sleight of hand which appealed to specialists in their own day but which had little appeal to later generations.

The fact that our work takes the form it does, pictures in a frame, stained glass windows and so on, is purely the result of custom and convention, and there is no reason at all in my mind why the emphasis should not change from the painted picture to some other form, and most artists are agreed that the opening made by Walt Disney presents untold possibilities. It might be interesting to note in passing, though it contains a whole subject in itself, that at a time when would-be painters would like to throw craftsmanship over altogether, Disney, by organizing other people's craftsmanship on a great scale and supplying the original urge himself, is achieving wonderful things.

(2) What are the inner experiences which prompt and guide invention? Speaking for myself, it seems that they are largely visual, but two kinds of vision are involved. First there is visual experience of seeing a scene, or combination of figures, or an attitude of a single figure, which sets up a craving to arrest that movement for all time, to catch the lovely rhythm that one knows by experience will be gone in a moment. It may be a combination of line, of mass or of colour that lights the imagination, or it may be only one at a time, but something seems to flash to the brain. Sometimes the origin is purely mental and may happen in the middle of the night. The second kind of vision is that while working on a picture or a design, even where a very definite inspiration may be lacking, during the growth of the work on canvas or paper, one sees combinations and suggestions that themselves become a fresh inspiration and so lead to the completion of the work.

In the vision of nature inspiration is essentially a personal vision. If I am out with a companion, say a brother artist, and draw his attention to the objects which inspire my idea, he either does not see them or sees something different which may be just as good as mine or better, but is quite definitely not the same. The same fact was also proved by taking a photograph where there was no time or opportunity to make a sketch. It is a curious fact that no such photograph was the least use, it was invariably dull and prosaic, whereas I had seen something vivid and exciting. The conclusion seems to be that when inspiration of this kind comes it is not the objects that one sees, but the combination of a number of things together, which sets up a kind of rhythm, and it is this rhythm which is the inspiration. Now this rhythm is not purely decorative, it may contain elements of character, of style or of any emotion which may be expressed graphically.

The other kind of vision may perhaps be explained by thinking of looking at a plaster wall where the faint differences of tone suggest scenes of battle or other things that follow one another till the whole wall is peopled with crowded incident, earth, sea and sky. A fresh untouched canvas, where one can see the warp and woof crossing one another, suggests the same sort of thing. When I was a boy of six or seven my bedroom had blue linen blinds, and when the light came through them in the morning I could see woven in them scenes which far excelled the pictures in my story books. But it is an interesting point that these scenes did follow the lines of what I knew from other pictures combined with what I could see for myself in the country. That is one reason for suspecting the originality of many clever children's work. They draw knights and fairies and princesses, but from their earliest babyhood it is almost impossible to have escaped seeing what others have imagined along these lines. Therefore much so-called original work is in reality secondhand, and you are more likely to find real original work by the boy or girl who tries to draw his granny by the fireside, or even a Spitfire which he has seen. Again, sometimes you find the very small child trying to draw God, and this shows the real original impulse, for to see a picture of God is so rare that a child is very unlikely to have come across it.

The problem of what kind of person the artist has to be in order to create is a very large question, but you would find amongst most artists the same differences of character as in any profession. You will find provident and improvident, irascible and kindly, lacking in self-confidence, supremely self-confident, and nearly all the phases of general character which are to be found among men. It is more difficult to pin down the resemblances, but a certain inner intensity which cares far more for the making of things than for any result which may accrue from selling them, and a sympathy and appreciation of the world around, would seem to be two of the most pervading resemblances. A mind that is quickly set in motion by suggestion from nature or thought, that can range far and wide on possibilities and can follow it up by the necessary craftsmanship, is more likely to be creative than the purely dreamer mind which is so often associated by the public with the creative artist. It is true that one sometimes meets the dreamer who produces little, but that little of a highly creative quality; this is however rare, and the dreamer type of artist is more commonly one who appears to have all the necessary qualifications except that of continued executive power.

(3) We may ask what is the precise form in which inspiration actually presents itself. Very shortly but not very satisfactorily answered, it is usually in the form of a picture.

Once having got the idea for a picture, it seems to be imagined automatically in terms of paint and canvas, the size and general proportions being important factors. The first idea is often of the slightest, a mere motive of general direction, pattern and colour, bounded by the rectangular or other shape which is contemplated. This first idea is highly exciting or emotional, and can be kept in the head and recalled at will until the work is actually begun. Then an interesting thing happens. When the first slight idea is transferred to paper or canvas, the second kind of vision begins to take charge, and the actual development of the work suggests criticisms and modifications unforeseen in the mental image. The reverse process takes place at the end of the day or during the night when recalling the work without its physical presence, and improvements or changes suggest themselves. One of the most annoying things is that what appear to be logical and necessary improvements do not turn out to be improvements at all, and one comes face to face with the fact that one does not understand one's own work and that the best efforts would appear to come from something working through and not consciously controlled by one's power. This may be put down to the difference between thought expressed in words and thought expressed in line and colour. It is impossible to transfer one to the other, and the genuine painter's power of thought in line and colour as carried out by his hand is superior to his thought in words then passed on to his hands. It is an undoubted fact that however carefully a picture is planned it invariably takes charge and may lead to an unexpected result. It would be safe to say that no result turns out exactly as planned or ever comes up to the first exciting and emotional conception, and so we turn expectantly to the next. It is also impossible to judge the work until the heat and excitement of doing it have died down, and the work can be judged in a quiet and dispassionate manner.

More affection is often held by work which has meant the greatest struggle and is at the same time the greatest failure, than for the obvious success. Such a work seen in later years when it is possible to adopt a detached point of view frequently brings a revulsion of feeling in the opposite direction and leads to destruction. Other works will be held in a curiously undeserved affection, which perhaps cannot be justified except that they permit the recapture in a modified form of the original thrill of inspiration. Again a work that has had general acceptance by the public, if passed as genuine by the artist, falls into a category quite of its own, and shortcomings obvious to him are overlooked.

(4) How far does one paint in a form which actually fits and fulfils given technical and artistic requirements, or in a way which may break them in a manner which turns out to be creative and original? Twenty years ago no doubt my answer would have been different, but after reaching a certain stage in life it seems that my conscious efforts to break away from traditions and conventions have not been successful. Though throughout my painting life I have made experiments in abstraction they have not been shown in public. Some of my other efforts were thought to be daring in my youth, but they would only be thought old fashioned to-day, a fate that is reserved for everything that does not rise above fashion. Only those things which, seemingly original in their time, are yet found to be in the mainstream of tradition, can survive, and this can be found in any gallery, where the works of van Gogh, Cézanne or Whistler rest peacefully on the walls with Fra Angelico, Velasquez or Rembrandt, and all are found to contain the same qualities in a different mixture. There will be others to join that illustrious

gathering, whose worth is perhaps not yet revealed to us. But it should be noted that van Gogh, Cézanne and Whistler all stressed tradition, though each saw it in his own way, and time has proved that they saw tradition more clearly than their contemporaries who in endeavouring to follow it had only chased a fashion.

Cheerfully, therefore, I accept the limitations of paint-brushes and canvas, and try to reduce inspiration to some sort of order by the use of these things. The use of paint has many lovely qualities that have been explored, and no doubt there are others still to try, but pictorial composition is finding new means of expression on the cinema screen and elsewhere, and the same sort of integrity, purity and simplicity of mind, which kept the masters so close to nature that they could catch her secrets, will guide the new masters also. The genuine painter does not try to paint objects as objects, but as part of a relationship of colour and form which expresses something. This may, in the case of a portrait, express character; and the character of the sitter, in whom the artist is obviously interested, will dictate something of the decorative relationships to be arrived at in order to express the subject.

It is possible to finish on the note on which we began. Technical limitations are accepted because the painter is a workman, and a good workman will have explored his tools and found just what he can do. As to conventions, every artist who puts a frame round his picture and hangs it on a wall is accepting a convention, and it would be very hard to say where conventions end and originality begins. It is my inclination to say that I only use those conventions which are generally accepted and which in no way affect the value of a work of art. As a workman I have the greatest respect for my tools and medium and would not willingly use them in a way that would seem to be abuse and almost cruelty to inanimate things that are in themselves beautiful.

My greatest thrill comes from seeing, my greatest pleasure from getting down to a job of work, with no thought of meals or breaks, and probably my greatest disappointment comes when it all ends in a picture no better than I have done before.

IV. Conclusion. By R. W. Pickford

The conclusions to be drawn from the two contributions to this symposium may be presented in generalized form under the two headings of individual and social aspects of psychology. The generalized form does not mean that we are able to speak of all artists in general. It is far from the intention of this symposium to do any such thing. The generalizations simply mean that a kind of verbal shorthand has been used in drawing together the points of particular interest which emerge most clearly. It is striking that Mr Hutchison and Prof. Whittaker, in spite of wide differences from each other in some respects, and although they did not collaborate at all in drawing up their statements, have reached very many points of agreement. These points of agreement will be apparent in the conclusions now to be drawn.

(1) In considering the social psychology of the artist, it is clear that the creative painter or musician is greatly influenced by the social environment in which he is placed. Music is greatly indebted to folk songs and dances, which are the great natural reservoirs of musical interest among the people of every land. We notice this to-day because music had worked itself out to a certain aridity of character in the late classical developments of the nineteenth century. Many modern composers have felt a new inspiration in

returning to folk tunes for material. It is, however, still true that many of the greatest classical composers utilized folk music and were by no means independent of it.

In the sphere of painting a new set of valuations for art might be developed if we could realize more clearly that the artist is a person whose craft contributes to the daily life of the public, or at least might be able to do so if allowed. This has perhaps been forgotten with the enthusiasm for adventure into the realms of individual fantasy, which began in the nineteenth century and may possibly terminate with Surrealism. This has, psychologically speaking, carried individual fantasy to its ultimate conclusion by making every easel picture a reflexion of the artist's own unconscious. The painter, even of the most outstanding individuality, however, such as Cézanne, turns out on careful study to have been more true to the real forward drift of cultural tradition than were apparently more conventional contemporaries.

Apart from the dependence of the original artist upon the background of folk art and traditional thought and feeling, we must think of him as a contributor of something valuable to people's daily lives. This form of dependence was stressed by Mr Hutchison in his view of the artist as a man who does a job of work for clients and patrons, and by Prof. Whittaker in his idea that composition would be pointless and indeed impossible apart from the urge to make music for other people. The one view is perhaps commercial, the other idealistic; but both are integrated and reflect the very important psychological truth that human daily life is essentially a life of interests which may be personal but which are followed out expressly because of their value to fellow-men. The artist or musician who divorces himself from this social existence will find a refuge in the mental home.

(2) We may now turn to the individual psychology of the artist. In a very brief way it is possible to indicate that the creative artists who contributed to this discussion are deeply anxious to make their art the medium through which they are understood and valued, and wish to avoid futile attempts to explain themselves in other terms. Each feels that words are useless substitutes for his chosen medium. This reluctance to exhibit themselves in the wrong way is not due to a false modesty, of course, but to a conviction that in their respective arts they have each found the true methods of expression for themselves, and that to attempt to use another medium would be a betrayal of their own powers. It is also true that music and painting are, in the sense indicated in the previous paragraph, objectified interests, deeds by which a man may be known, whereas introspection is ineffective as a source of personal satisfaction and regressive because it leads away from the social justification of daily life. The unwillingness to try to substitute words for music or painting also illustrates an interesting psychological principle, that certain kinds of material are in some way appropriate to certain ideas, feelings and experiences, and have a peculiar fitness also to certain personalities. It is thus true that, despite their own difficulties in saying what particular kind of man a musician or a painter must be to create, both the contributors to this symposium have clearly shown that he must have at least one outstanding quality: the composer is one who thinks, feels and constructs in terms of music; the artist in terms of paint or pencil and canvas or paper.

The creative artist is singularly individual in his own peculiar modes of working. His peculiarities of experience and of vision have been pointed out clearly in the foregoing pages. The artist or composer is not able to say definitely where his inspiration comes from. It is dependent on the existence of a demand for his work to a certain extent,

but he will probably tend to find ways of expressing his artistic ideas even when there is little or no demand. Inspiration in music may be very closely related to the presence of performers whose activities suggest more and better ideas and inventions. It may depend in pictorial art on the unexpected discovery of stimulating combinations of objects in the environment, which excite a desire to express this vision on paper or canvas. That such experiences are genuinely individual is clear, because photographs of the same objects are useless as substitutes for the preliminary sketch, and even artistic companions do not see the objects just in the way the particular painter sees them himself. In music, surroundings may also unexpectedly fill the mind with inventions and stimulate a freedom from restraint which allows of rapid and effective composition. Artificially induced conditions of excitement are not effective in the same way and are not used to any extent so far as we can say. Like the scientific research worker the creative artist may have to endure weeks or months during which no ideas for new work come to him. When such ideas do come, they are sometimes successful, but it is quite possible to find that one has composed pages of would-be music which must be put on the fire when inspected in the light of cold consideration. Even when a vivid idea has been conceived, the power to work hard and persistently at the labour of actual writing out or detailed brushwork is essential if a good product is to be completed. In making pictures or music it seems often true that the work of art, once begun, may take charge itself and, however definite ideas the artist entertained about it beforehand, may lead the way in unexpected directions. It is impossible to estimate the real merit of a work until the excitement of making it has cooled and one can take a detached view.

Two different kinds of artistic vision have been described. In the first kind some object or combination of objects sets going in the mind of the artist a peculiar rhythm of feeling and imagination which causes him to seek to make his inspiration permanent in picture form. In the other kind the canvas itself, or a plaster wall with its roughnesses, or the very act of working on a picture itself, tends to fill the mind with pictorial ideas projected upon the canvas or material.

In music, the inspiration presents itself in musical terms; in painting, in pictorial terms. To the composer his inspiration is of music; to the painter it is a picture. Apart from these barriers, beyond which psychological analysis has in this symposium failed to take us, it is nevertheless possible to say that inspiration seems to begin by being vague in form, though definite and emotionally exciting, and to tend to expand itself in one's mind, partly by the individual allowing himself to become saturated in it and partly through his efforts to work out the final product in detail by successive steps. In picture making it seems possible to complete a work in various degrees of superficial perfection. Mr Hutchison says that while a brief sketch may satisfy the artist himself on some occasions, ordinarily he is impelled to carry on to the utmost of his ability. Social conventions, however, may also demand a more finished product, which conforms to the standards accepted in contemporary art. This has varied at different times, and, for instance, since the Impressionists and Whistler the standard of superficial finish has changed greatly. In the matter of what might be called the personal demand for artistic finish artists differ widely, and it is fair to mention a man like Cézanne, who was a somewhat clumsy technician, but brought his pictures to a high degree of finish in his own peculiar manner, without regard for social demands. Most of van Gogh's canvases were completed in a single day, but were much more than brief sketches.

The musician is inspired by the feel of the sounds made by instruments and voices. To hear a gifted player make single notes and scales on a fine instrument may be a most delightful and exciting experience, because it suggests vast possibilities of musical expression. In the same way the painter is excited by the feel of the brush and of the sticky paint, by the colours laid out on his palette and by the smell of the oil. All these qualities make him want to work with these materials and tools, and to employ them constructively. Love for the sounds or paints in themselves is an essential of creative inspiration, and the artist respects and appreciates his tools and materials just like a craftsman in metal or wood. He hates to see them undervalued or falsely employed. The scientific worker has the same love for his apparatus and the writer for his pen and ink.

This conclusion is not a substitute for the individual accounts by Prof. Whittaker and Mr Hutchison themselves, but may serve to emphasize the psychological interest of what they have said. It is hoped that the reader will be able to compare the views they give in this symposium with his own experiences in creative work, or with those accounts given by other artists, and thus gain some insight into the problems of individual and social psychology in artistic work.

Appendix. By R. W. Pickford

The sections on musical and pictorial art which make up the main part of this paper were contributed by Prof. W. Gillies Whittaker and Mr William O. Hutchison to a symposium on 'Psychological Aspects of Pictorial and Musical Art', at a meeting of the Scottish Branch of the British Psychological Society on 6 December 1941 in Glasgow. These authors very graciously allowed me to adapt their material for publication in its present form. Their contributions were made in response to a questionnaire drawn up by myself and adjusted by Dr Boris Semeonoff. These questions were formulated in order to cast as wide and indefinite a net as possible, which would also suggest precise problems, and it will be seen that the same problems are sometimes posed in different words. Prof. Whittaker and Mr Hutchison have responded to the intention of these questions, and have answered them freely, as desired, and not slavishly. The questionnaire is printed below because it is of interest in relation to this symposium, and might offer help to another psychologist dealing with a similar problem.

The Questionnaire

It has been suggested that the lecturer should concentrate chiefly on giving a first-hand account of his own experiences, methods and ideas, though comparisons with those of other people would be interesting if brought in occasionally. The following notes are a guide, but need not be followed in detail. Illustrations in the form of pictures or on the piano would be welcome.

(1) The first question in which we are interested is about the objective circumstances which prompt you as a creative artist. How is your invention stimulated by other people's interest, by commissions, by customs and conventions, by folk art, by scenery and buildings and such objects, by the availability of suitable performers of music and by the expected audience or the people who are to look at pictures? How far is the expectation of an audience essential to your art?

(2) The second question is about the kind of inner experiences which prompt and guide invention. What is your urge to compose or to paint connected with in the way of personal experiences? What do you as an artist or musician feel you are trying to express, or what, in your own private and emotional life, underlies the desire to express something of artistic interest to other people? What kind of person does the artist have to be in order to create, or what kind of personality is characteristic of the composer, if any?

(3) The third question is about the precise form in which your inspiration actually presents itself. Is it strongly visual or auditory, or is it thought out in abstract terms? Is it highly charged with

emotion or not? Is it violently insistent or can it be delayed and criticized? Is the piano necessary to you as a composer? How far do you as a painter or musician foresee exactly what you will produce? To what extent is the product a surprise, and do you view it as your own willed composition or as something which came from a source which is impersonal or outside the scope of your own conscious ego?

(4) Finally, we are anxious to know how far you compose or paint in a form which actually fits and fulfils given technical requirements, or in a way which may break them in a manner which turns out to be creative and original. How far does the composition or work of art have to be adapted, either on paper or in mind, to fulfil such demands after or during construction? Is the adaptation done consciously or is it automatic? Why is it necessary to adopt conventional forms and symbols at all?

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HEAD'S CONCEPT OF THE SCHEMA AND ITS APPLICATION IN CONTEMPORARY BRITISH PSYCHOLOGY

PART II. CRITICAL ANALYSIS OF HEAD'S THEORY

By R. C. OLDFIELD and O. L. ZANGWILL

I. *Critique of Head's theory* (pp. 58–60): (1) *Material and functional aspects of the schema* (pp. 58–59); (2) *The building of schemata* (pp. 59–60); (3) *Schemata and the mechanism of response* (p. 60); (4) *The physical basis of schemata* (p. 60). II. *Bartlett's interpretation of Head* (pp. 60–63). *References* (p. 64).

I. Critique of Head's theory

Systematic and minute criticism will, as we have already explained, find no place in this essay. Nevertheless, it will perhaps be advisable to pause at this point in our exposition, and try to indicate briefly the chief respects in which Head's theory offers difficulties within its own field of explanation.

(1) *Material and functional aspects of the schema.* Perhaps the most salient difficulty arises when we attempt to envisage the relations between what may conveniently be called the *material* and the *functional* aspects of the schema. This is, indeed, a form of difficulty that arises in all theories of the manner in which the reaction of the organism is co-determined by present stimulus and past events. By the *functional* aspect of the schema, we intend reference to the fact that it is a mechanism serving to mediate perception and thereby to determine response in a manner consonant with the adaptive requirements of the organism. And we may refer to this aspect of the schema without any accompanying implications regarding the particular mode of existence that belongs to it, or the particular manner in which events in the past have contributed towards its construction and maintenance. On the other hand, so soon as an attempt is made to see more closely into the nature of the mechanism, it is scarcely possible to avoid the assumption that in some sense or other a record of past events, or of the environment to which the organism reacts, is involved.[1] Whether this be in the form of specific traces of past excitations, or of a generalized schematic model or picture built up upon the basis of those excitations, is a matter of theoretical difference. This aspect of the scheme, the fact that it incorporates a 'record', will be spoken of as the *material* aspect.

It is in the material aspect of the schema that we find much both of the novelty and of the difficulty of Head's approach. It is novel in that he tends to view the record of the past not as the whole, nor even as the major part, of the whole mechanism, but simply as one essential feature of it. It is novel especially in rejecting the view that the mere passive recording of traces, which in some simple direct way are supposed to

[1] Prof. Bartlett objects to our use of the word 'record' as being unfair to Head. While we concede without hesitation that much that is most valuable in Head's treatment depends on his abandonment of the strictly representational position, we cannot see our way to denying that the schema *does* involve a record in the sense in which we use the term. It would certainly be wrong to define the schema *as* a record, yet the use of a term such as 'self-compensating standard', which well expresses its functional attributes, hardly does justice to those material aspects which Head's own treatment makes it necessary to distinguish. We have therefore allowed our word 'record' to stand, but only in the hope that others may thereby be stimulated to discover a fitter and fairer substitute.

represent past events, can afford a sufficient basis for the presentation of data to consciousness.

Nevertheless, it is plain that a record of past events must at least formally be incorporated in the schema, even though such record bear no more immediate resemblance to that which it records than does Newton's *Principia* to the solar system. And it is evident that, while postulating a peculiar form of such record, Head has not considered closely the question of the relations in general between records and prototypes. At times he speaks as if the schema were but an organized body of impressions. Thus he refers to the cortex as "the storehouse of past impressions", and these impressions may, he believes, rise into consciousness as images, although more often they remain outside it, "where they form organized models of ourselves" ((4), p. 607). In saying this, as Bartlett points out, Head gives away too much to the 'storehouse' idea. Doubtless in doing so he was concerned to find some means of accounting for images. But it is difficult to see what it is that determines that an impression shall rise into consciousness as an image. It is not clear, either, whether it is possible for one and the same impression to belong to an organized model, *and* to be available for access to consciousness. It appears to us that Head would have been more consistent had he rejected the 'storehouse' notion altogether, and attempted to find some other means of accounting for images. In this matter he seems to have been affected by the traditional but naïve standpoint which maintains that, because images are so highly specific and determinate, they must arise through some more or less simple and direct process of communication with a specific trace. But it may well be that, strikingly photographic though they sometimes are, images are the product of an essentially complex, possibly 'reconstructive', process, and owe their faithfulness and specificity rather to the excellence of the reconstruction than to the simplicity of the process by which they are aroused.

It is evident at least that Head is constrained to make use of the notion of the schema in its material aspect in order adequately to describe its functions. To some extent these two aspects are at variance with one another. We believe that fuller consideration of the nature of the schema in its material aspect is urgently demanded. A possible line of inquiry might lie in the closer study of imagery in patients with defective postural appreciation. Such a study, if unfettered by preoccupation with that simplest of modes of representation—the purely pictorial—might well afford a basis for the removal of much that is at present obscure in Head's presentation.

(2) *The building of schemata.* Head frequently refers to the *building* of organized models, but nowhere gives any clear account of the means whereby this operation is performed. A simple trace theory would demand no account of this process beyond the general statement of how any event is recorded in trace form. But the schema is no mere concatenation of traces, and is so specialized and peculiar an entity that it is impossible to imagine that the mere existence of impressions in the storehouse could spontaneously, and without active intervention of some factor external to them, generate a model of the kind Head postulates.

One might hope that data relevant to this question would be forthcoming from two sources, namely, (1) studies of cases in which the schemata are not 'abolished' but only 'disturbed' as a result of cortical lesions; and (2) studies of the process of regeneration of function where recovery from a cortical lesion takes place. Head gives no clear analysis of the appreciation of posture in cases where the lesion has not resulted in *total* loss of

the power to recognize position and passive movement. His attitude seems to be that if such appreciation is defective, the schemata are 'disturbed'. But there is no indication of what, precisely, is the nature of this disturbance.

(3) *Schemata and the mechanism of response.* It is necessary to emphasize the fact that the schema does not in itself provide the means of describing the whole process of response determination. It only allows us to envisage, in terms more plausible than those of a trace theory, those conditions, relating to afferent processes, which must hold if influences from the periphery of the organism are to play their part adequately in the determination of response. But the statement of these conditions proceeds no further than the "highest afferent centres". The nature of the processes by which the product of reaction between schema and incoming impulses on the one hand determines overt response, and on the other can give rise to judgement, or awareness of change, is left unexplained. It may be that Head had in mind some direct influence of the sensory upon the motor centres. But we feel that the matter requires further elucidation, especially in relation to the function of the schemata in maintaining static tone. Here, as we have seen, a constant stream of influence is said to be exerted upon the highest efferent centres by "...*the simple existence* of the physiological dispositions which we have called schemata" (italics ours).

(4) *The physical basis of schemata.* Head, it is plain, is under no obligation to enter more minutely than he does into the intimate nature of the physiological processes and dispositions upon which depend the existence and operation of the schemata. On the other hand, it is quite legitimate for others to raise the question of how we are to try to envisage, in terms of actual cortical processes, the formation, continued existence, and functioning of the schema. Here again the corresponding problem for the trace theory raises by comparison but little prima facie difficulty.

Into this question we cannot enter here, but it is perhaps worth while pointing out that the continued existence of the schema, at times when it is not functioning in the production of overt response, must probably be viewed in terms rather of a continued physiological process than of a persistent modification of structure. For, in the first place, the mere lowering of vigilance (an essentially physiological process) will suffice to impair or destroy schemata. In the second place the simple *existence* of the spatial schemata is said to exert that constant stream of influence upon the highest efferent centres which determines static tone. It is difficult to imagine how the simple existence of a special anatomical structure could have such an effect entirely upon its own account.

II. Bartlett's interpretation of Head

The first indication of Bartlett's extension of the concept of the schema is to be found in his critical review(1) of Head's *Aphasia and Kindred Disorders of Speech.* This review represents in part a summary and critique of Head's general position, and in part an anticipation of the important views subsequently developed in detail by Bartlett in his book *Remembering*(3).

Bartlett presents a definition of the 'schema' practically identical with that of Head. "First", he writes, "the organism responds only to immediately presented external stimuli. But there is no observable case in which the conditioning of the response can be stated in terms of a narrowly isolable stimulus. *The response depends upon an arrangement of preceding responses which have already been organized.* The name given to these

organized responses, which may produce their effects in the entire absence of any awareness of them on the part of the reagent, is *schema.* | At any level of physiological response a number of these schemata must be assumed to be in operation" ((1), p. 584, italics ours).

There are three points in Bartlett's presentation which call for comment. In the first place he speaks of the schema as comprising a series of organized past *responses*, while in Head's conception they consist in organized past *impressions*. This may not, however, constitute any great difference of opinion. Bartlett would presumably include in the response in question those physiological states which, brought about by the stimulating external situation, in their turn determine the overt response. Head, on the other hand, would certainly include under the term 'past impressions' those which relate to bodily events involved in the giving of overt responses.

In the second place, Bartlett refrains from laying any emphasis upon the character of the schema as *model*. Head certainly intended his expression 'schema' to indicate an arrangement of preceding impressions, but it was an arrangement of a very highly specialized character—"a model of ourselves which constantly changes".

Thirdly, Bartlett categorically states that a number of these schemata must be assumed to be in operation at *any* level of physiological response. Head, on the contrary, stipulates expressly that the elaboration of spatial schemata depends upon processes occurring only at the level of *epicritic* integration. There is no indication in Head's writings that schemata are to be supposed to operate at lower functional levels in the nervous system. It is implied that they are a form of mechanism peculiar to the highest levels.

In his discussion of 'matching', imaging, and rote recall, Bartlett indubitably goes beyond Head's original theoretical framework. Here we find not only an attempt to resolve some of the psychological problems left unanswered by Head, but also a definite anticipation of the line of thought subsequently developed so brilliantly in *Remembering* (3). "Now the simplest order of arrangement of the effects of these sensory responses into *schemata*", writes Bartlett, "is the direct sequential or serial order. Every sensory response contributes to the determination of the next succeeding response of the same mode. But, more than this, a response in one mode, say visual, can be directly equated with a response in a different mode, say tactual. This is what Head calls 'matching'" ((1), p. 584).

Discussion of the problem of matching is outside the scope of the present paper. It is, however, necessary to point out that Bartlett lays special emphasis upon what we may call the organization of the sensory schemata on the basis of common sensory modality. His postural schemata, in fact, appear to be built out of touch, pressure, and kinaesthetic impressions. All that Head explicitly states in this connexion is that the appreciation of position and the power of surface localization necessitate reference to different schemata. It is possible that Bartlett's emphasis upon modal organization may derive from this distinction.

In his treatment of images, Bartlett offers some constructive suggestions as to the relation of images to schemata, and here also, for the first time, we find an indication of the view that the organization of *psychological* (i.e. experiential) material may be 'schematic' in character. "In 'matching'", Bartlett writes, "symbols first begin to be used; one sensory pattern stands for or symbolizes another. Such sensory symbols, however, involve a very complete repetition of the details of the original presentation for which they stand, and cannot operate if there is much of a time gap between them

and the original. A step forward is taken by the development of 'images'. To use mental images, according to Head's treatment, is to develop a device for picking concrete items out of the setting in which they originally occurred, and for utilizing them irrespective of the precise sequential order to which they actually belonged in the first place" ((1), p. 584).

The term 'setting', as used by Head,[1] is not, it is true, synonymous with the term 'schema', but it is evident that Bartlett has a schematic theory of the organization of past experience in view. He remarks that "...it does not matter whether the *schemata* which come into play are those highly complicated reactions called 'conscious' and dealt with by the psychologist, or the relatively less complex and earlier reactions frequently termed 'reflex' and dealt with by the physiologist. *Their mode of controlling simpler responses is always the same, and 'vigilance' is a factor the general character and functions of which persist throughout the whole history of organic and mental life*" ((1), pp. 585–6, italics ours). Bartlett has not, it is true, attempted to explain precisely how it is that the purely *physiological* dispositions postulated by Head are able to condition the appearance of sensations and images in consciousness.[2] He has, however, indicated that the concept of the schema may profitably be applied to the problem of the organization of past experience at the conscious level. This conclusion is further borne out by his statement that 'imaging', the picking out of items from their original settings, is a relatively high-level accomplishment, to be distinguished sharply from relatively low-level "repetitive memory". On the lower level, if breakdown occur, a series of reactions must be begun over again from the beginning and carried out in fixed serial order if the difficulty is to be overcome. "This case", he adds, "is, for example, constantly illustrated by the child reciter" ((1), p. 585). The trend of Bartlett's argument suggests that, at all events on a low level, the organization of past experiences, or of past reactions, is to be envisaged as an organization built up in the same way, and possessing the same general characteristics, as Head's postural schemata.

In his critique of the concept of 'vigilance', Bartlett rightly points out that, according to Head, what reaction is evoked in response to a given stimulus depends partially on the nature of the response in relation to the schemata, and partly on a general vital state of the central nervous system (vigilance), by virtue of which the latter can exercise more or less complete selective control. Bartlett is in agreement with Head when he states that, at a high degree of vigilance, it is the "most complex, lately acquired and highly integrated *schemata*" that have functional control over "the less complex, earlier, and less highly integrated types of organic response" ((1), p. 585). But he goes beyond Head in his theory of *hierarchical* disposition of the schemata.[3] On this view, there are

[1] Cf. "The image is the product of a higher form of mental activity. It is a piece of experience which is taken out of its settings and employed as a method of defining a situation where some practical difficulty of adjustment has arisen" ((5), p. 538).

[2] Cf. "But...the 'sensory' cortex registers and retains the physiological dispositions produced by past events. These profoundly modify all subsequent actions. *They may be manifest in the form assumed by sensations or images*, but more *often*, as in the case of spacial impressions, remain outside consciousness. *Here they form organised models of ourselves which we termed 'schemata'*" ((5), p. 435, italics ours).

[3] Head did, of course, apply the concept of functional levels to psychological activities. High-grade performances on the conscious level he supposed to necessitate a high degree of psycho-physiological vigilance, and maintained that they might be disturbed or abolished by any condition resulting in lowering of physiological vitality. Thus, of mechanical skills he writes that "...these aptitudes are profoundly influenced by conditions of general health or by anything which lowers physiological vitality" ((5), p. 487). More generally:

both high-level and low-level schemata, and in states of diminished vigilance the control of the former over the latter may lapse, or even, in the case of permanent cortical injury, be entirely abolished.

Yet another feature introduced by Bartlett, and one which may, perhaps, in some degree circumvent the difficulty, unsurmounted by Head, of the relation between image and schema, lies in his characterization of the latter as manifesting both *dynamic* and *material* aspects. Of the dynamic character of schematic activity, Bartlett writes that "...building the *schema* is an active process; linking new incoming stimuli with their appropriate *schemata* is an active process; interrelating the various *schemata* is an active process; the control of the *schemata* over less highly integrated organizations is an active process" ((1), p. 587). And whilst the schema is to be regarded on the one hand as an arrangement of responses, "...it is also, regarded from another point of view, an arrangement of material sensory at a lowly level, affective at a perhaps slightly higher level, imaginal at a higher level yet, and eventually ideational and conceptual. *But its functions as arrangement of responses are not to be confused with its character and functions as arrangement of material*" ((1), p. 587, italics ours).

It is evident that Bartlett's review incorporates far more than what even the most liberal 'reading between the lines' of Head's writings can disclose. The most salient extension of the schema theory is that in which it is applied to the elucidation, not of the highest levels of organic activity per se, for Head himself envisaged such application in cases of functional disintegration, but of those aspects of normal activity that can be described only in experiential terms. If Head did not himself thus extend the notion of the schema, it may well be because, for his purpose—the elucidation of pathological phenomena—there was no immediate need for such extension.[1]

"Every automatic act demands retentiveness and can be disturbed by vital states, which have nothing to do with consciousness. What wonder that the complex powers demanded by speech, reading and writing, can be affected by a lesion, which diminishes neural vitality. Vigilance is lowered, and the specific mental aptitudes die out as an electric lamp is extinguished, when the voltage falls below the necessary level" (loc. cit.). Head's view of the relations subsisting between consciousness and physiological process in general are obscure. In his writings he was evidently much influenced by the fact that he was dealing with *breakdown* of function. The products of such breakdown, he insists, manifest no fundamental distinction of mind and body. "The mind-body problem does not exist so long as we are examining the consequences of functional disintegration" ((5), p. 544). Bartlett appears to be exclusively responsible for the *experiential* applications of the concept of schemata. Head nowhere explicitly states that the high-grade conscious activities and processes are *themselves* schematic in character, nor does he suggest that the organization of past experience involves mechanisms formally similar to those which he holds responsible for the maintenance and regulation of posture. The only explicit indication that Head was prepared to sponsor the thesis that schemata may operate on different functional levels is to be found in a footnote on p. 488 of *Aphasia* (5). "It must not be forgotten", he observes, "that the theory of schemata involves two conceptions, the schema building and the schema built. *The first is an activity corresponding to all those processes discussed in this work*" (i.e. 'matching, imaging, symbolic formulation and expression', etc.; italics ours), "the second is a state which results from this activity". But Bartlett undoubtedly goes farther than Head in his inference that "Schemata may be built up in many different ways and the differences may turn out to be psychologically far more important than the fact that, however different they are, *they can all be called 'schemata', and all be treated as having the same general characteristics and effects*" ((2), p. 160).

[1] We have Prof. Bartlett's authority to mention that he and Sir Henry frequently discussed the general and theoretical problems raised in *Aphasia* (5) during the period of its writing, and that the discussion was not confined to those aspects of the theory which are explicitly dealt with in Head's writings. Sir Henry makes mention of the part played by these discussions in his preface to *Aphasia*. Prof. Bartlett has also told us that Sir Henry regarded the review in *Brain* (1) as an accurate interpretation of the views which he had endeavoured to express in *Aphasia*.

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PUBLICATIONS RECENTLY RECEIVED

Psychoanalytic Method and the Doctrine of Freud. By ROLAND DALBIEZ. Translated by T. F. LINDSAY, with an Introduction by E. B. STRAUSS. London: Longmans Ltd. 1941. 2 vols. 40*s.* net.

M. Dalbiez has undertaken the difficult and much-needed task of discussing the methods and doctrines of psychoanalysis as contained in the writings of the late Dr Sigmund Freud. In the present circumstances, this excellent translation by Mr T. F. Lindsay is most opportune. Approaching the subject from a philosophical standpoint, it was not long before the author discovered that philosophy alone is insufficient to resolve the new problems set by psychoanalysis, and that before discussing the assertions of Freud it was necessary to begin by reconstituting the same experiences. Together with this, M. Dalbiez endeavours to sift with the aid of philosophical criteria the theoretical concepts which Freud employs, esteeming that, in what concerns human knowledge, if nothing begins without experience, nothing is achieved by it alone.

Pursuing the dual path of observation and reflexion, it is seen that in psychoanalysis taken as a whole one has to distinguish a method, a psychology and a philosophy. The failure on the part of many exponents and critics of the subject to keep these distinctions clearly in view has led to much confusion of thought and endless controversies. M. Dalbiez has therefore divided his study into two parts, in the first of which will be found the exposition or 'setting forth' of psychoanalysis presented in as favourable a light as possible, to do justice to its principal author. Only too frequently, it is observed, a travesty of Freudian teaching is presented, and this substitute doctrine is then set up for discussion and criticism. Commencing with the psychopathology of everyday life, the author treats in turn the subject of dreams, the sexual theory, the theory of the neuroses and psychoses, ending with sublimation, art and religion. The second part is devoted to discussion of the methods and theories of psychoanalysis.

The author has throughout endeavoured to maintain an attitude of strict impartiality, and to give as accurate an account of Freudian teaching as possible. In this he has succeeded, and in making clear much that is obscure; for, as he remarks on many occasions, a great deal of the prevailing misunderstanding of psychoanalysis is in no small measure due to the obscurity of Freud's exposition of his own views, as well as to a certain indifference in furnishing convincing proofs of his assertions.

Since no summary of these volumes can do justice to their contents, we can here only briefly comment on certain points which the author himself considers to be fundamental. The first place is taken by the doctrine of the unconscious, which together with that of dreams is the centre of psychoanalysis. In fact it is this doctrine, even more than the sexual theories of Freud, which has aroused the most opposition, especially from the adherents of radical empiricism and idealist philosophy. The unconscious, it is here maintained, is not only an essential concept for psychoanalysis, but also for psychology; without it there cannot even be a psychology. M. Dalbiez has his own views as to the nature of the psychical unconscious, with which he compares the views of Freud, finding indeed many points of agreement as well as much with which he disagrees. Ultimately the discussion revolves round his own philosophical standpoint of moderate realism, which is in his view the better philosophical instrument, for it does afford an intelligible explanation of the unity of the human ego sedulously upheld by Freud. Discussing the methods of exploring the unconscious (vol. II), M. Dalbiez maintains with Jung that the application of the term 'symbol' to "those conscious contents which give us a clue, as it were, to the unconscious backgrounds are by Freud incorrectly termed symbols. These are not true symbols, however, since, according to his teaching, they have merely the role of signs or symptoms of the background processes." It is thought that much of the opposition to the psychoanalytical interpretation of dreams is largely due to the confusion created by the use of the word *symbol* in the sense of *index* or *effect-sign.*

Taken as a whole, these volumes are a valuable commentary on the Freudian contribution to both normal and pathological psychology. G. A. E.

War in the Mind. By CHARLES BERG. London: The Macaulay Press. 1941. Pp. 272. 8*s.* 6*d.* net.

It is a pity that the practice of collecting a number of separately published articles together, and calling them a book, should be perpetuated, especially in these days of paper shortage. Had the author brought forward these case histories as the raw data for a logical exposition of the theory and practice of psycho-

analysis, he might have produced a really valuable book on the subject. No doubt as they stand the case histories will be useful to lecturers for their illustrative material. But to anyone who has no previous knowledge of the psychoanalytic theory, and must wait until he reads the last chapter of the book to gain it, the case material may be entertaining enough, but its interpretation, other than a purely superficial one, will seem hopelessly far-fetched. In other words, it is useless to put case material first and theory after—or vice versa; they must be coherently welded together to produce understanding and conviction. And one is sad to see such good case material so ably described yet so badly wasted.

The Psychology of C. G. Jung. By JOLAN JACOBI. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1942. Pp. xi+169. 12*s.* net.

A clear and relatively brief exposition of the Jungian theory has long been needed. For the reader who prefers a broad uncritical outline to a detailed and systematic scientific study, this book may fulfil this need. Such a reader must be content to try and understand the theory intuitively, helped by illustrations from a few briefly described practical instances. The book includes no detailed evidence from case histories, dream series and myths; thus it is impossible by reading it to verify the logical derivation of the theory from such evidence. It may be of course that no such derivation is possible for a theory which sets itself to appeal to intuitive understanding rather than to critical reasoning. In this case, acceptance of the theory leaves the realm of scientific inquiry and enters that of metaphysics or of faith. It appears that the way of understanding is probably for a few potential initiates rather than for the ordinary scientific inquirer. To the former this book may be recommended as an introduction to the subject, which would, however, be useless unless it were followed by a study of Jung's own longer and more detailed works. But perhaps of all psychological theories it can be said most truly of this one: "A little learning is a dangerous thing."

Change No. 2. Home Propaganda. A Report prepared by MASS-OBSERVATION. London: the Advertising Service Guild. 1942. Pp. vi+78. 1*s.*

The second number of *Change*, the Bulletin of the Advertising Service Guild, marks an advance to a subject of much greater importance than that of the first number, namely, to that of Home Propaganda; that is to say, of official propaganda addressed to the people of this country since the beginning of the war. This study deals mainly with posters, leaflets and Press campaigns; but the effects of radio, films, lectures and meetings are also discussed. It rightly emphasizes the fact that what people do in response to propaganda is often different from what they say they think about it, and is much more important. What they say has been studied by the questionnaire method described in the first number of *Change*; but, as was pointed out before, the number of people questioned was too small and their distribution too narrowly confined, to give really reliable results. For conclusions about actual behaviour, Mr Harrisson, the director of Mass-Observation, appears to rely largely upon the information given by his "nation-wide panel of voluntary informants". Their factual information, e.g. as to the frequency with which gas-masks are carried, may be reliable enough. But one is left feeling a little uncertain as to the nature of these sources of information, and how far they accurately determine the conclusions.

There is no doubt, however, as to the importance of these conclusions, and one must admire the manner in which they are stated and discussed. The ineptitude and inefficacy of most official propaganda need stressing in season and out of season. So also do the conclusions that appeals should be far fewer, less diffuse and less mutually conflicting; that they should be much more clearly and simply stated; and that official leaders should as far as possible instruct the citizen in what he *is* to do, and why, rather than *pleading* with him to do it. "An appeal is intended for questions of sentiment or private preference. For bigger and more urgent matters *instruction* is what the civilian needs, indeed expects. And he can take it TOUGH—and like it."

Disease and the Social System. By ARTHUR GUIRDHAM. London: George Allen and Unwin, Ltd. 1942. Pp. xvi+239. 10*s.* 6*d.* net.

The author of this book advances the thesis that the trend in modern society is towards an enormous increase in functional nervous disorders, particularly the psychoneuroses, which more than offsets the decline in organic disorders such as tuberculosis, and in epidemic diseases. A large proportion of somatic symptoms, e.g. of rheumatism and of digestive disorders, are mainly psychological in origin and are produced by the intense mental strain of modern civilized life. For such cases Dr Guirdham prescribes

first and foremost more rest and relaxation in simple unstrained pleasures (once the Germans are safely beaten and subjugated!). But modern hygienic and medical practice have also resulted in a preservation of the unfit, particularly of the neurotic character which he thinks to be incurable.

Many of the points that he makes are interesting and suggestive. But unfortunately they appear to be based, not even upon documentary evidence of case records, but upon random recollection and hearsay. He gives no case histories, no aetiological studies, no quotations from published writings, and no relative frequencies of the various pathological states and symptoms which would demonstrate the relationship he postulates between specific types of neurotic disorder and specific somatic complaints. He pours scorn on the use of scientific method in medical practice. That is as may be; but without the factual evidence afforded by scientific observation and treatment of the results of observation, his thesis must remain within the realm of speculation and conjecture. Moreover, his somewhat dogmatic pronouncements as to prevention and cure will be accepted with the utmost reserve until there is more definite evidence of their prognostic value. M. D. V.

The Effect of Supervision on High School English. By Howard Andrew Kinhart. The Johns Hopkins University Studies in Education, No. 30. Baltimore: The Johns Hopkins Press. 1941. Pp. ix+102. $1.25.

This monograph describes an experiment in which the improvement of pupils in English was compared for groups the teachers of which were in some cases given special supervision, and in some cases not. The supervision consisted of advising the teachers to use elaborately prepared methods of grading the work given to the pupils according to their capabilities, together with discussions of methods and of especially difficult cases among the pupils. On the whole, the increase in 'educational age' (in English tests) was greater for the groups the teachers of which were supervised than for the control groups. The difference was not very striking. The reader is left with the impression that the method of supervision, with its elaborate test technique, would befoozle an unskilled teacher but would be ignored by a skilful one.

The Probability Integral. By W. F. Sheppard. British Association Mathematical Tables, vol. vii. Cambridge University Press. 1939. Pp. xi+34. 8*s*. 6*d*. net.

The tables in this volume comprise:

I. The ratio of the tail area of the normal curve to its bounding ordinate to twelve decimal places, at intervals of one-hundredth of the standard deviation.

II. The same ratio at intervals of one-tenth of the standard deviation, to twenty-four decimal places.

III. The negative natural logarithm of the tail area of the normal curve to twenty-four decimal places, for integral multiples of the standard deviation.

IV. The negative natural logarithm of the tail area of the normal curve at intervals of one-tenth the standard deviation, to sixteen decimal places.

V. The common logarithm of the tail area of the normal curve at intervals of one-tenth the standard deviation, to twelve decimal places.

VI. The common logarithm of the tail area of the normal curve at intervals of one-hundredth of the standard deviation, to eight decimal places.

All the tables go up to ten times the standard deviation, and the necessary reduced derivatives, etc., for interpolation are given.

COUNCIL ON HUMAN RELATIONS

INVITATION TO COLLABORATORS

THE COUNCIL ON HUMAN RELATIONS is interested in furthering collaboration among all students of personality and culture whose collected materials, current researches, and projects of research may be useful for a scientific approach to problems of international relations. In particular, the Council is interested in the immediate problems of co-operation among the allied nations and the future problems of world reorganization, when the co-operation of all nations will be necessary.

The Council is requesting the following information:

1. Names of persons who are:
   (*a*) Carrying on such researches.
   (*b*) Have carried on such researches.
   (*c*) Have raw materials or facilities for such researches.
2. Descriptions of bodies of collected materials which are relevant to the personality and culture problem, especially in the major contemporary cultures of the world, both occidental and oriental, such as:
   (*a*) Collections of autobiographies.
   (*b*) Case histories, personality studies, and systematic observations on individuals or groups of individuals.
   (*c*) Results of psychological testing applied to members of different cultures in a standardized way.
   (*d*) Comparable bodies of data on any aspect of contemporary culture, especially bodies of data which have been comparably collected from more than one such culture, e.g. collections of advertising symbols, legal practices and their implications, artistic forms, philosophical systems, etc., that may illuminate cultural emphases and the differences between the various cultures.
   (*e*) Collected data on family patterns and relations and on child rearing and development.
   (*f*) Collected material on the behaviour of minority groups, which may throw light on the cultures of origin of these groups and on the conditions under which members of these groups are able to live and work with members of other cultures.
3. Information about all proposals for current co-operation and post-war planning which explicitly recognize that members of different cultures have been reared under different sets of institutions, have different personality structures, and diverse ways of seeking social order.
4. Information about new techniques or new applications of existing techniques to the problem of cross-cultural differences in personality and character.

The Council hopes to serve as a clearing house for research in this broad field, by putting those who are interested in working on the same culture in touch with each other, and those who are working with comparable techniques, but on different cultures, in touch with each other. The Council also proposes to circulate among its collaborators abstracts and bibliographies on these subjects.

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